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THE PHILOSOPHICAL MAGAZINE:

COMPREHENDING

GEOLOGY,
THE VARIOUS BRANCHES OF SCIENCE,
THE LIBERAL AND FINE ARTS,
AGRICULTURE,
MANUFACTURES AND COMMERCE.

NUMBER CLXXI.

For JULY 1812.

CONTAINING THE FOLLOWING ENGRAVINGS:

1. A Plate to illustrate the Foundation of Mr. William Jones's temporary Corn Rick.
2. Mr. Stephens's Method of dividing Bricks.
3. Section of Mr. William Jones's temporary Corn Rick.
4. Mr. Waistell's Improvement on the Acorn Dibble.

BY ALEXANDER TILLOCH,
M.R.I.A. F.S.A. EDIN. AND PERTH, &c.

LONDON:
PRINTED BY RICHARD TAYLOR AND CO., SHOE LANE:
And sold by Richardsons; Cadell and Davies; Longman, Hurst, Rees, Orme & Brown; Murray; Highley; Sherwood and Co.; Harding; London: Constable and Co. Edinburgh: Brash and Reid, and D. Niven, Glasgow: and Gilbert and Hodges, Dublin.

Mathematical and Scientific Education.

MR. FIRMINGER, Assistant Astronomer at the Royal Observatory, Greenwich, from the Year 1798 to 1807, has commenced an Establishment for a limited Number of Young Gentlemen, resident in his House, Charles Street, Clarendon Square, Somers Town, for Instruction in the various Branches of the Mathematics and Mathematical Sciences, with their practical application to the several Departments of a Civil, Military, Naval, and Commercial Education.

The Courses of Instruction include, besides the Mathematics with their practical application to Civil, Military, and Naval Admeasurement, Geography, History, Literature, the Modern Languages, and Polite Accomplishments, in which the assistance of proper Masters will be employed.

A Series of Astronomical Observations will be made, including, besides such as are taken in conducting the Business of a Regular Observatory, those of Lunar Distances, Eclipses of Jupiter's Satellites, and all Observations requisite in determining the Longitude at Sea or Land, the Rates of Time keepers, &c. Whereby the Students of Navigation and Astronomy have the advantage of acquiring a complete knowledge of the Practical as well as Theoretical Departments of these Sciences.

A Course of Experiments in the several branches of Natural, Chemical, and Mechanical Philosophy, will be exhibited and illustrated to the Pupils, in which the Mathematical Formulæ derived from Theory and Induction, will, where the subject under consideration admits of such method of investigation, be applied and explained.

Surveying, Levelling, and the several Branches of Civil and Military Admeasurement will be practically illustrated by an application of the different Instruments appropriated to their respective subjects of investigation, with Drawing Plans, Maps, Projection, Perspective, &c.

The Works of Hutton, Playfair, Atwood, the Cambridge and different Courses of Mathematics, with such Authors, English and Foreign, as afford the best information, will be Books of Reference and Study in the respective Departments of Science which they contain.

The above Course of Studies and Establishment is calculated (and it is hoped will prove to those who honour it with their patronage) to afford the most effectual means for the Education of Young Gentlemen in the several branches of Instruction which are professed to be taught; and by which they will be prepared for the Professions of the Army and Navy, for their entrance into the Public Colleges and the Universities, and for any Department of Public or Private Life, wherein, besides a Classical, a Mathematical and Scientific Education is required.

Young Gentlemen preparing for their entrance at the University at Cambridge are instructed in the Elements of Euclid by Simson, the Cambridge Mathematics, and the Courses of Study adopted in the several Colleges of that University.

The above is, in part, the Outline of a Plan of Instruction drawn up about four years since by Mr. Firminger, for the late Right Hon Charles Greville, which was to have been adopted under Mr. Firminger's superintendance in a large Establishment preparing for that purpose at Milford in South Wales.

A most Complete and Elegant Observatory was nearly finished, and a set of Astronomical Instruments, purchased at great Expense, was provided in readiness for commencement, when unfortunately the Death of Mr. Greville, which happened suddenly, put a premature close to the undertaking, as sufficient funds (undoubtedly owing to so unexpected a circumstance) had not been provided for its completion.

Mr. Greville, whose sources of information were widely extended, and whose abilities to appreciate were acknowledged of superior merit, being fully impressed with a conviction that much was wanting in Schools of Education, and that by a proper allotment of their time the Eleves for Instruction might, in the usual period allotted to a Classical Education acquire also a considerable degree of Mathematical and Scientific Knowledge, Departments of Instruction he held, in the present enlightened state of Society, equally necessary in forming the Public and Private Character, had determined that nothing should be wanting in his Establishment to complete his intention of making it subservient to the acquirement of Mathematical and Scientific as well as of Classical Knowledge, for which purpose every necessary provision would have been made for the Experimental as well as Theoretical Departments of Science, had he lived to complete his intentions. As a more particular account of this intended Establishment and Plans of Education may be interesting and useful to the Public, an extended account will be given in a future Number of this Work, with a Plan and Elevation of the Observatory, which at the same time combines an Elegance of Form with Simplicity and Convenience, certainly not surpassed if equalled by any of the Observatories now extant.

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AGRICULTURE,
MANUFACTURES AND COMMERCE.

NUMBER CLXXII.

For AUGUST 1812.

CONTAINING THE FOLLOWING ENGRAVING:

A Plate to illustrate Mr. Tawney's New Thrashing Machine.

BY ALEXANDER TILLOCH,
M.R.I.A. F.S.A. EDIN. AND PERTH, &c.

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MANUFACTURES AND COMMERCE.

NUMBER CLXXIII.

For SEPTEMBER 1812.

ILLUSTRATED WITH THE FOLLOWING ENGRAVINGS:

Two Octavo Plates descriptive of a New Machine for Pumping Water.

BY ALEXANDER TILLOCH,
M.R.I.A. F.S.A. EDIN. AND PERTH, &c.

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AGRICULTURE,
MANUFACTURES AND COMMERCE.

NUMBER CLXXIV.

For OCTOBER 1812.

ILLUSTRATED WITH THE FOLLOWING ENGRAVINGS:

Two Plates descriptive of the Muscular Motion of Snakes.

BY ALEXANDER TILLOCH,
M.R.I.A. F.S.A. EDIN. AND PERTH, &c.

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ENGRAVINGS.

Vol. XXXV. A Head of M. HAUY, engraved by T. WOOLNOTH from an original Drawing by F. MASSARD.—A Plate illustrating M. HAUY's Crystallography.—Dr. WOLLASTON's Goniometer: Dr. HEALY's New Cupping Instrument: and a Diagram to illustrate Mr. WALKER's Theory of Vision.—A Plate to illustrate HAUY's Crystallography.—Mr. CAVENDISH's dividing Instrument.—New Electrical Apparatus.—A Plate to illustrate M. HAUY's System of Crystallography.—Mr. ACCUM's Hydro-pneumatic Table.—A Plate to illustrate M. HAUY's Crystallography.—Captain PASLEY's Telegraph, and Mr JOHNS's Apparatus for Decomposing Potash and Soda.—Diagrams to illustrate M. MONGE's Paper on the Composition and Decomposition of Forces.—A Plate to illustrate HAUY's Crystallography.—Two Plates of Apparatus employed by Mr. DAVY in the Electrochemical Experiments detailed in his Bakerian Lecture.

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AGRICULTURE,
MANUFACTURES AND COMMERCE.

NUMBER CLXXV.

For NOVEMBER 1812.

CONTAINING THE FOLLOWING ENGRAVINGS:

1. Figures to illustrate such Portions of a Sphere as have their Attraction expressed by an algebraic Quantity.—Van Helmont's Differential Thermometer.
2. Formation of Ovals for Gardeners.

BY ALEXANDER TILLOCH,
M.R.I.A. F.S.A. EDIN. AND PERTH, &c.

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AGRICULTURE,
MANUFACTURES AND COMMERCE.

NUMBER CLXXVI.

For DECEMBER 1812.

CONTAINING THE FOLLOWING ENGRAVINGS:

1. Chavasse's Marine Transit.
2. A Plate to illustrate Mr. Brodie's Experiments on the Influence of the Brain on the Generation of Animal Heat.

BY ALEXANDER TILLOCH,
M.R.I.A. F.S.A. EDIN. AND PERTH, &c.

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THEATRE OF ANATOMY,

Blenheim-Street, Great Marlborough-Street.

THE SPRING COURSE of LECTURES on ANATOMY, PHYSIOLOGY, and SURGERY, will be commenced on Monday, the 25th of January, at Two o'Clock,

By Mr. BROOKES.

In these Lectures the Structure of the Human Body will be demonstrated on recent Subjects, and further illustrated by Preparations, and the functions of the different Organs will be explained.

The Surgical operations are performed, and every part of Surgery so elucidated as may best tend to complete the operating Surgeon.

The Art of Injecting, and of making Anatomical Preparations, will be taught practically.

Gentlemen zealous in the pursuit of Zoology, will meet with uncommon opportunities of prosecuting their researches in Comparative Anatomy.

Surgeons in the Army and Navy may be assisted in renewing their Anatomical Knowledge, and every possible attention will be paid to their accommodation as well as instruction.

Anatomical Converzationes will be held weekly, when the different Subjects treated of will be discussed familiarly, and the Students' views forwarded.—To these none but Pupils can be admitted.

Spacious apartments, thoroughly ventilated, and replete with every convenience, are open all the Morning for the purposes of Dissecting and Injecting, where Mr. Brookes attends to direct the Students, and demonstrate the various parts as they appear on Dissection.

An extensive Museum, containing Preparations illustrative of every part of the Human Body, and its Diseases, appertains to this Theatre, to which Students will have occasional admittance.—Gentlemen inclined to support this School by contributing preternatural or morbid parts, subjects in Natural History, &c. (individually of little value to the possessors) may have the pleasure of seeing them preserved, arranged, and registered, with the Names of the Donors.

Terms.	*l.*	*s.*
For a Course of Lectures, including the Dissections,	5	5
For a perpetual Pupil to the Lectures and Dissections,	10	10

The Inconveniences usually attending Anatomical Investigations, are counteracted by an anti-septic Process, the result of Experiments made by Mr. Brookes on Human Subjects, at Paris, in the year 178 , the account of which was delivered to the Royal Society, and read on the 17th of June, 1784. This method has since been so far improved, that the florid colour of the Muscles is preserved, and even heightened. Pupils may be accommodated in the House. Gentlemen established in Practice, desirous of renewing their Anatomical Knowledge, may be accommodated with an Apartment to dissect in privately.

THE

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M.R.I.A. F.S.A. EDIN. AND PERTH, &c.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

VOL. XL.

For JULY, AUGUST, SEPTEMBER, OCTOBER, NOVEMBER, and DECEMBER, 1812.

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THE

THE

PHILOSOPHICAL MAGAZINE.

I. *Experiments which prove Platina, when combined with Gold and Silver, to be soluble in Nitric Acid.* By Mr. PERCIVAL JOHNSON.

To Mr. Tilloch.

SIR, DURING the practice of a profession where much depends on depriving gold of its alloys, having heard it suggested that platina might be used as an advantageous one, under the idea of its being but slightly affected by the nitric acid, I determined on a closer inspection of its action on that metal than had hitherto been described. The insertion of the following remarks on my experiments will oblige me.

I find, although platina when in a pure state is not acted upon by the nitric acid, that when alloyed with gold and silver it is perfectly soluble, and with silver alone partially so.

From 1 to 15 per cent. of platina to the gold was entirely dissolved, leaving the gold a good colour and perfectly pure, having previously mixed the gold with three times its weight of silver for quartation.

Twenty per cent. lost $\frac{7}{12}$ths, leaving the cornet flaked and black inside; 30 per cent. lost $\frac{4}{9}$ths with the same appearances.

The buttons have an indented crystalline and red appearance after cupellation, more so in proportion to the quantity of platina, and in the two latter proportions are rounded at the edges.

By mixing double the quantity of silver the gold was freed of upwards of 20 per cent. of platina, and more according to the quantity of silver employed; and yet the gold seems to be a necessary component for the perfect solution; for by mixing the following proportions of *silver* and *platina*, the results were thus:

Platina and silver equal to 1 per cent. of the former left in dilute acid a light powder partly subsiding. 5 per cent. gave the acid a straw colour, half the platina employed being dissolved. 10 per cent. a very bright straw colour, having dissolved the same proportion.

15 per cent. a bright light brown, having dissolved $\frac{4}{7}$ths of the platina. 25 per cent. a deep brown, dissolving $\frac{2}{3}$ds of the platina.

The two latter proportions required concentrated acid after the action of the dilute.

Maiden Lane, Wood Street, July 1, 1812.

PERCIVAL JOHNSON, Assayer of Metals.

It may also be worthy the notice of your readers, that we find palladium to be such a general alloy of Brazil gold as often to alter the colour thereof. We have particularly observed it in the Brazil coin, many of which were rejected at first sight, suspecting them to be counterfeits. We found it a short time since in a Brazil bar to the amount of nearly 20 per cent. altering the colour thereof to nearly that of the metal *palladium*.

J. AND P. J.

II. *An Account of some Experiments on the Combinations of different Metals and Chlorine, &c. By* JOHN DAVY, *Esq. Communicated by Sir* HUMPHRY DAVY, *Knt. LL.D. Sec. R. S.**

Introduction.

MY brother, Sir Humphry Davy, appears to me to have demonstrated, in his last Bakerian Lecture, the existence of a class of bodies similar to metallic oxides, and consisting of metals in union with chlorine or oxymuriatic acid.

These combinations are the principal subject of the following pages. I shall do myself the honour of giving an account of the experiments I have made to ascertain the proportions of their constituent parts, and likewise of describing some that have not yet been noticed.

I shall have to relate also the attempts I have made to ascertain the proportions of sulphur in several sulphurets, and the experiments I have performed to estimate the quan-

* From Philosophical Transactions for 1812, part i.

tity

tity of oxygen in some metallic oxides. The general analogy of definite proportions led me to both these undertakings. This analogy, it will be perceived, I have constantly kept in view, and have had recourse to, both for detecting inaccuracies in my own experiments, and in considering the results of the experiments of others.

As the nomenclature connected with the old hypothesis, respecting oxymuriatic acid, is inconsistent with the new views of this substance, I shall venture to call the compounds of the metals and chlorine to be treated of, by the names which my brother has proposed for them.

1. *On the Combinations of Chlorine and Copper, &c.*

There are two distinct combinations of chlorine and copper. both of which may be directly made by the combustion of this metal in chlorine gas. When the gas was admitted into an exhausted retort containing copper filings, the filings became ignited, a fixed fusible substance quickly formed, and the interior of the retort soon became lined with a fine yellowish brown sublimate. The former substance evidently contains least chlorine; for when it was heated alone in chlorine gas, it absorbed an additional portion, and was converted into the latter. Hence the fixed compound may, in conformity with the principles of Sir Humphry Davy's nomenclature, be called cuprane, and the yellow sublimate, cupranea.

Cuprane may be procured in several other ways. It may be obtained by heating together copper filings and corrosive sublimate; and it was thus first discovered by Boyle, who called it resin of copper, from its similitude to common resin. Two parts of corrosive sublimate, and one part of copper filings, I have found the best proportions of the materials.

It may be obtained by boiling copper filings in muriatic acid, or by exposing slips of copper partially immersed in this acid to the atmosphere. In the last instance, I have found the changes connected with the formation of cuprane rather complicated; the copper exposed receives oxygen from the atmosphere, and acid from the ascending muriatic acid fumes, and is thus converted into a green insoluble salt; and this absorbing more muriatic acid, slowly passes into the deliquescent muriat, which flowing into the muriatic acid is changed by the action of the immersed copper into cuprane.

M. Proust, the first modern chemist who examined cuprane, and who is commonly considered as the first dis-

 coverer

coverer of this compound, found it produced by the action of muriat of tin on muriat of copper; he named it white muriat of copper, and ascertained that a similar substance results from the decomposition of the common deliquescent muriat by heat.

Cuprane, by whatever means prepared, possesses the same properties. It is fusible at a heat just below that of redness, and in a close vessel, or a vessel with a very small orifice, it is not decomposed or sublimed by a strong red heat; but if air, on the contrary, is freely admitted, it is dissipated in dense white fumes. It is insoluble in water. It effervesces in nitric acid. It silently dissolves in muriatic acid, from which it may be separated by the addition of water, which precipitates it unaltered; and it is decomposed by a solution of potash; or by heating it with the fused hydrated alkali; when it affords the orange oxide of copper. Its colour, transparency, and texture appear alone to vary. It is generally opake, of a dark-brown colour, and of a confused hackly texture; but I have obtained it by cooling it slowly after it has been strongly heated, of a light yellow colour, semi-transparent, and crystallized, apparently in small plates.

Cupranea is only very slowly formed by heating cuprane in chlorine gas. The best mode that I have found, of procuring it, is by slowly evaporating to dryness, at a temperature not much above 400° of Fahrenheit, the deliquescent muriat of copper. Thus made, it has the same appearance, and the same properties, as when directly formed. It is of a yellow colour, and pulverulent. Exposed to the atmosphere, it is converted, by the action and absorption of water, into the deliquescent muriat, and its colour, during this alteration, changes from yellow first to white, and lastly to green. It is decomposed by heat; and even in chlorine gas when the experiment is made on a pretty large quantity, part of the chlorine is expelled, and assumes the gaseous state, and cuprane remains.

I have employed the same methods for ascertaining the proportions of the constituent parts of both these combinations. I have separated the copper by iron, and the chlorine by means of nitrat of silver.

A solution of 60 grains of cuprane in nitro-muriatic acid, precipitated by iron, afforded 51·2 grains of copper, well washed, and perfectly dried.

A solution of the same quantity of cuprane in nitric acid, precipitated by nitrat of silver, afforded 117·5 grains of horn silver dried, till it ceased to suffer any loss of weight

weight by exposure to a temperature above 500 Fahrenheit.

Since horn silver contains 24·5 per cent. of chlorine*, 80 grains of cuprane appear to contain 51·2 grains of copper and 28·8 of chlorine. And 100 appear to consist of

36 chlorine
64 copper

100

A solution of 40 grains of cupranea in water, acidulated with muriatic acid, precipitated by iron afforded 18·8 grains of copper.

And a solution of 20 grains of cupranea in water, precipitated by nitrat of silver, afforded 43 grains of horn silver.

Hence 100 of cupranea, omitting the very slight loss, appear to consist of

53 chlorine
47 copper

100

The deliquescent muriat and the native muriat of copper of Peru, belong to a class of compounds apparently distinct from the preceding combinations of copper and chlorine.

The deliquescent salt is well understood; and its composition may be inferred, independent of its water, from that of cupranea.

The native muriat is less known, I shall therefore relate the experiments I have made on this interesting mineral.

The specimen I have examined is part of a very fine one, presented to Sir Humphry Davy by William Jacob, Esq. M.P. and deposited in the Museum of the Royal Institution. It consists of muriat and carbonat of copper, of red oxide of iron, and of green coloured quartz. The muriat is partly crystallized; the crystals, from the trials I have made of them, appeared to be pure, and they were, on that account, made the subject of my experiments.

The crystallized muriat dissolves entirely and without effervescence, in all the acids in which I have tried it, and the deliquescent muriat of copper is in each instance formed, and a combination of brown oxide of copper with the acid employed.

* This I have ascertained by synthesis; 12 grains of pure silver dissolved in nitric acid, and precipitated with muriat of ammonia, yielded 15·9 grains of fused horn silver. I do not give the particulars of the experiment, which was very carefully made; because the result very nearly agrees with that of Klaproth, and of other chemists.

Heated slowly in a bent luted glass tube, connected with mercury, the native muriat affords water and oxygen gas, and the residue is an agglutinated brownish mass, which dissolves in muriatic acid and gives a greenish precipitate with potash, and is apparently a mixture of brown oxide of copper and cuprane. When the heat is raised rapidly to redness, the water expelled is impregnated with muriatic acid and muriat of copper. I have obtained from 25 grains of the mineral heated to redness till gas ceased to be produced, just two cubic inches of oxygen. This expulsion of oxygen seems to be owing to the action of chlorine on the brown oxide to form cuprane; and there is, I have ascertained, a similar production of oxygen when heat is applied to a mixture of the deliquescent muriat and brown oxide of copper.

From these results, which perfectly agree with those obtained by eminent chemists on the continent, who have examined different specimens of this mineral, it appears to be a submuriat of copper, differing in a chemical point of view from the deliquescent salt, merely in containing a smaller proportion of acid.

The following experiments were made with the design of ascertaining the proportions of its constituent parts.

Fifty grains of the crystals in powder, boiled in a solution of 50 grains of potash, afforded 36·5 grains of brown oxide of copper heated to dull redness.

And 20 grains dissolved in nitric acid, and precipitated by means of nitrat of silver, afforded 12·9 grains of dry horn silver.

Hence, considering the deficiency of weight, as indicating the quantity of combined water, 100 of the native submuriat of copper seem to consist of

73·0 brown oxide		15·8025 chlorine
16·2 muriatic acid	=	
10·8 water		·47 hydrogen.

This analysis, allowance being made for difference of theory, nearly agrees with that of Klaproth.

M. Proust, I believe, first discovered an artificial compound similar to the native sub-muriat of copper. He obtained it, in the preparation of the nitro-muriat of copper, and also by a partial abstraction of the acid of the deliquescent muriat, by means of an alkali. I have found that it may be procured in several other ways. It may be made directly by adding the hydrated blue oxide of copper to a solution of muriat of copper; and it may be very readily and œconomically prepared, by exposing to the

the atmosphere slips of copper partially immersed in muriatic acid; and it is also produced by the exposure of cuprane to the atmosphere. Its production in the last instance is accompanied with that of the deliquescent muriat; and the formation of both seems to be owing to the absorption of water and oxygen; for cuprane, I have found, though apparently not in the least acted on by dry oxygen gas, is quickly changed when moistened with water and confined in a jar of this gas, and there is a rapid absorption of oxygen*.

I have not examined all the specimens obtained by these different methods minutely, though sufficiently, I conceive, to ascertain their identity, and their similarity to the native compound. The colour of all of them is greenish white, like that of the native, in a finely divided state. When heated, they all afford water, oxygen gas, and a mixture of cuprane and brown oxide of copper.

I have analysed only the submuriat, precipitated from a solution of muriat of copper by a weak solution of potash.

Fifty grains of this, well washed and dried, boiled in a solution of potash, afforded 36·3 grains of dried brown oxide of copper.

And 20 grains dissolved in nitric acid, and precipitated by nitrat of silver, afforded 12·75 grains of dried horn silver. These results differ so little from those obtained with the native, as fairly to permit the conclusion, that the composition of the artificial and native submuriat of copper is the same.

2. *On the Combinations of Tin and Chlorine, &c.*

Tin, like copper, is capable of combining with two different proportions of chlorine. The liquor of Libavius, one of the combinations, is directly formed by the combustion of the metal in chlorine gas; and the other, I find, may be produced by heating together an amalgam of tin and calomel. Thus obtained, it is similar to that which may be procured by evaporating to dryness the muriat containing the gray oxide of tin, and fusing the residue in a close vessel. Both are of a gray colour, and of a resinous lustre and fracture, and both inflame, like tin itself, when heated in chlorine gas, and are converted into the liquor of Libavius by the absorption of a fresh portion of chlorine.

* I have been informed that submuriat of copper is sometimes found in the neighbourhood of volcanoes, particularly in that of Vesuvius. By means of the above facts, it is evident that its production might be accounted for in such situations.

Hence,

Hence, as the liquor of Libavius contains the largest proportion of chlorine, it may be called stannanea, and the other compound stannane.

Stannane is fusible at a heat below that of dull redness; it bears this temperature, if air be nearly excluded, without undergoing any change; but when subjected to a heat as strong as glass will bear without being fused, it appears to be, from the slight fume produced, partially decomposed.

It affords the liquor of Libavius when heated with corrosive sublimate, nitre, red oxide of mercury, or with the hyperoxymuriat of potash. In the three last instances, oxide of tin is also formed; and with the hyperoxymuriat the action is so violent that inflammation is actually produced.

The liquor of Libavius and aurum musivum are formed when stannane is heated with sulphur.

Stannane, by the action of water, appears to be converted into the insoluble submuriat of tin, and the acidulous muriat.

The stannanea or liquor of Libavius, that I have examined, was made by heating together an amalgam of tin and corrosive sublimate, in the proportions commonly recommended. I have obtained this compound in another way, by treating the concentrated solution of the peroxide of tin in muriatic acid, with strong sulphuric acid; a gentle heat applied to this mixture, contained in a retort, expels the fuming liquor, which may be condensed, as usual, in a cold receiver.

The only new and remarkable property which I have observed the liquor of Libavius to possess, is its action on oil of turpentine. I was led to make trial of it from an idea of Sir Humphry Davy, that the combinations of the metals and chlorine might be soluble in oils. In the first experiment, when I poured the fuming liquor into the oil, inflammation immediately took place, with violent ebullition and production of dense reddish fumes. I have used other specimens of oil of turpentine, expecting a similar inflammation, but without its occurrence, though there has been in every instance a considerable action. The mixture of the two being made in a retort connected with mercury, no gas was generated, oxide of tin appeared to be formed, and a viscid oil was produced, which, like the fat oils, left a permanent stain on paper, and had little smell or taste, and which, digested with alcohol, imparted something which occasioned a permanent cloudiness on the admixture of water, and an odour to me not unlike that of artificial camphor.

camphor. The action of the liquor of Libavius on the oil of turpentine is worthy of further inquiry. The preceding account of it, I am aware, is very incomplete; but I trust it will serve to call the attention of chemists to a subject so curious.

To discover the proportions of tin, and consequently of chlorine, in stannane and stannanea, I have taken advantage of the superior affinity of zinc for chlorine, by means of which the tin is separated in its metallic state.

69·5 grains of stannane, made by heating in a glass tube with a very small orifice an amalgam of tin with calomel, were, with the exception of two grains of metallic mercury, apparently a mere mechanical mixture, entirely dissolved in dilute muriatic acid. A slip of clean zinc, immersed in this solution decanted from the residual mercury, quickly precipitated the tin in a very beautiful plumose form; and this precipitate collected on a filter, and well washed and dried and fused into one globule under a cover of tallow in a small glass tube, weighed 42 grains.

As therefore 67·5 grains of stannane contain 42 grains of tin, 100 appear to consist of

62·22	tin
37·78	chlorine
100·00	

As stannanea is extremely volatile, it is difficult to weigh it with perfect accuracy. The mode I adopted, was to pour it into a bottle half full of water, the weight of which was previously ascertained, and to infer the quantity added by the increase of weight.

81·75 grains of stannanea, thus weighed in water*, afforded when decomposed by zinc 34 grains of tin.

Hence 100 of stannanea appear to be composed of

42·1	tin
57·9	chlorine
100 0.	

I am not acquainted with any analytical method for directly ascertaining the proportion of chlorine in either of the two preceding combinations. Nitrat of silver, when immediately applied, will not answer the purpose, because the

* A little muriatic acid was added before the zinc was introduced, to dissolve the oxide of zinc which, in other similar experiments, I observed was rapidly formed, and which, from the large quantity of hydrogen evolved, appeared to be owing to the decomposition of water, chiefly in consequence of the Galvanic effect of the contact of the two different metals, zinc and tin.

the oxide of silver is partially reduced by the solution of stannane; and an oxide of tin is thrown down in mixture with the horn silver from the liquor of Libavius.

M. Proust, to whom we are indebted for very excellent investigations of the different combinations of copper and tin, first discovered a submuriat of tin. He found that a solution of potash precipitated from the solution of muriat of tin this compound, and not the pure gray oxide of tin.

I have obtained it by his method, and all its properties which I have observed are perfectly agreeable to its supposed composition.

It is decomposed by a red heat. Subjected to distillation in a small bent glass tube connected with mercury, no gas was produced, water containing muriatic acid and muriat of tin was expelled, and a sublimate like stannane was formed, and the fixed residue was gray oxide of tin.

It effervesces violently with nitric acid; and strong sulphuric acid expels from it muriatic acid fumes.

It dissolves without effervescence in the muriatic and acetic and in the dilute, nitric, and sulphuric acids; and all these acid solutions, as they give a black precipitate with a solution of corrosive sublimate, appear to contain the tin in the state of gray oxide.

The complete analysis of this submuriat of tin is difficult. The oxide it contains cannot be accurately separated by potash, nor can nitrat of silver be employed to ascertain the proportion of muriatic acid.

I have found 50 grains of it, dissolved in muriatic acid, to afford, when precipitated by zinc, 31 grains of metallic tin. Now as this submuriat is similar to the submuriat of copper, the analogy being imperfect only in the latter containing the peroxide, and the former the protoxide, it is natural to infer that the proportion of muriatic acid is similar in both. But the proportion of muriatic acid in the submuriat of copper is apparently half of that which exists in the muriat: hence, supposing the composition of the submuriat of tin to be similar, 100 of it will consist of

70·4	gray oxide
19·0	muriatic acid
10·6	water
100·0.	

Probability alone can be attached to this estimate. I have not given the calculations by which it was made, as their data are liable to objection.

[To be continued.]

III. *On*

III. *On the Structure of the Earth* *By a* Correspondent.

To Mr. Tilloch.

Sir, In the present uncertainty which is extended over geological science, and the great diversity of opinion which prevails even with regard to its first principles, it is not to circumstances of local particularity that our attention should be directed so much as to facts universal in extent, and which point towards a knowledge, however imperfect, of the internal construction of our globe. It has indeed been well remarked, that the inclination of its superficial strata enables us to extend our views far beneath the limits of human labour; yet, even taking this into the account, we cannot but confess our inability to pierce beyond the surface. Mathematicians have determined 12 miles for the mean depth of the ocean; while the continents, so far as our knowledge extends, appear to consist of substances of from 2 to $2\frac{1}{2}$ times the density of water, and to rest on granitic masses whose density does not exceed this limit. The superficial density of our globe may therefore be stated at about $1\frac{1}{2}$. Yet the mean density is known to amount to nearly five times that of water. The great mass therefore which composes the nucleus of the earth must have about this density. With this fact appears intimately connected a question of great importance on the formation of mineral veins, whence is derived the matter which occupies them? A Neptunist will say, From above, by infiltration; while those who maintain the Plutonic system answer, From below, by injection, when fused. The arguments on both sides have been handled with great ability; but the density of many of these extraneous masses affords one which, though hitherto unnoticed, appears to me at once to decide their origin. If it be from above, how happens it that the *source* above, *from whence* it introduced itself, has never in one single instance been discovered? *Perhaps* the supply is already expended; *perhaps* it has already been carried down, with the rest of the *debris* of the soil, to the ocean. Still, however, its existence is hypothetical. On the other hand, the Huttonian explanation is subject to no such embarrassing queries. The density of metallic veins, deviating in so remarkable a manner from every surrounding phænomenon, directs us immediately to look for their source in the great reservoir which we know to contain all the more solid parts of our planet.——It is evidently

no

no answer to this argument to state that many veins, which traverse the strata, contain no metallic substances. As long as one instance can be found where this is the case, its force remains unbroken.

In hopes that these remarks may not be unacceptable to those of your readers who are partial to geological researches,

I remain, sir, yours, &c.

July 1, 1812. H.

P. S.—We may likewise remark, from the consideration of the great density of the earth, that the opinion of many philosophers, that granite forms its solid nucleus, must be unfounded.

IV. *New Method of making Bricks, so as to form cheaper and firmer Buildings, and useful underground Drains. By* John Stephens, *Esq. of Reading**.

Sir, I have sent, for the inspection of the Society of Arts, &c. three closure bricks, which on examination you will find to have been cut three-fourths of the way through in the middle by a wire, and the whole of the way through at each end, which leaves the ends square and handsome for work.

The bricklayer to divide each brick in length has only to take the brick in his left hand with the mark or cut downwards longitudinally, and by one smart blow with the trowel he will have two complete king closures, with which he can easily make four common closures.

I have shown them to many workmen, who all approve of them. I had two hundred and fifty of them made by a brickmaker for an experiment, and I have ordered two thousand more. The builders who do the principal part of my work have had some on their own account, and have since increased their orders. I have no doubt when they are better known they will come into general use.

A considerable saving in labour and waste of bricks may be effected by their use, particularly in walls where piers are built, and where there are many openings; the work will also be rendered more substantial.

There will be a saving in room and materials where the back of a chimney is built against a straight wall, particularly in flues for low buildings.

* From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce,* for 1811.——The silver medal of the Society was voted to Mr. Stephens for this communication, and specimens of the bricks are preserved in the Society's Repository.

They

They will be found useful in cities or large towns by being placed in partition walls instead of lath and plaster, and be a check to the ravages of fire.

They will be useful in preventing the passage of rats and mice, and the disagreeable smell occasioned when they die betwixt lath and plaster or wainscot.

They will also answer for draining land, and will form cheaper small drains from houses than any other method.

They may be cut in other forms or directions for particular purposes according to the uses for which they are intended.

The additional expense of dividing them by the wire is about two shillings per thousand; it is generally done after they have been moulded one or two days, according to the dryness of the season.

I flatter myself that, if this communication meets with the approbation of the Society, it will render a benefit to the public.

I am, sir, with much esteem,
Your most obedient servant,
Reading, October 31, 1810. JOHN STEPHENS.

To C. Taylor, M.D. Sec.

DEAR SIR,—ON inquiry from builders, I am informed, that the saving by the use of the bricks I have invented, will be from two-and-half to nearly five per cent. in a five-window house in brick work and labour, in a front of forty feet with or without piers.

In ornamental brick piers for gateways, I think the saving of bricks by means of cutting may be very considerable, and in the labour still more, besides the work being done more sound and substantial.

I am using a few of them in an eleven-inch brick-wall, (a system hitherto entirely new,) in a westernly aspect, as a preventative or guard against the effects of weather, and it will, in point of dryness, be equal to a fourteen-inch wall. I have inclosed a letter from Benjamin Garroway, a bricklayer, who has requested me to let him have all the bricks I have of this kind, and to bespeak more for him. I have also sent a certificate from Mr. Robert Wright, who is extensively engaged in buildings.

The drains for agricultural purposes might be done by women or children, except the digging of the drains, especially two-inch drains. With respect to longer drains, if they are required of four inches, and to be covered with brick,

brick, I would recommend the bricks to be laid anglewise, in order to promote strength in covering.

It would be of great importance if parliament would allow a drawback of the duty on all bricks employed in draining.

Every brick intended for the operation I recommend, is taken off the stack two or three days after it is moulded. It is then put on a stool or board, and a wire about the size of No. 23 is pressed on the upper side of the brick, so as to pass through each end of it; it is then immediately placed on the stack again, and afterwards burned.

I am, dear sir,

Your most obedient humble servant,

Reading, December 8, 1810. JOHN STEPHENS.

To C. Taylor, M.D. Sec.

Description of the Drawings of Mr. STEPHENS's *Method of cutting Bricks for various Purposes.* See Plate I. fig. 2.

Fig. 2. of Plate I. is a plan of the upper surface of a common brick; the line *a a* is a cleft cut nearly through the brick while it is soft by means of a piece of wire, as is shown in the section fig. 3, where the section of brick is shown at BB, placed on the wooden block A; a piece of wire *b b* with a loop at each end is pressed down into it, so as to divide it into two parts, except the part C, which the wire will not cut through because of the curvature it acquires in being pressed into the brick. A brick of this kind being burnt, may be broken in two halves by one cleft with the trowel, which will be found very useful in many cases which constantly occur in brickwork, and will be far superior to the present mode of hacking the bricks, both for the soundness and appearance of the work, and will be done in less time.

Figs. 4. and 5. show the application of these divided bricks to draining, where AB are the ends of the two halves of a brick, and CD tiles, forming the top and bottom of the drain: this method forms a square drain.

Fig. 5. shows how a triangular drain may be made with half the number of bricks of the foregoing, that is one-half brick A, and two tiles CD.

Fig. 6. is a plan of a brick divided diagonally, and fig. 7. shows how these halves may be disposed to form a triangular drain; the letters show the same parts in each of these two figures; the bottom D may be made of tile, or of a brick cut in half in its thickness: the scale annexed to the figures will show the dimensions of the different drains.

V. On

V. *On the Derivation of the Scale, &c.*

To Mr. Tilloch.

Sir, Dr. Crotch has recently published a quarto Treatise on Musical Composition, of which the last thirteen pages (from p. 124 to the end) treat on "the derivation of the scale, tuning, temperament, the monochord, &c." He remarks that "no two major keys at all *related* to each other can exist, on the same keyed instrument, perfectly in tune;" and adds that in a lecture on this subject, he caused the keys of E major with four sharps, and E flat major with three flats, to be tuned "perfectly" (perfect) on the same piano-forte; namely, first the triads E, G*, B—A, C*, E—B, D*, F*; and then, having two notes G* and D*, already tuned, which would serve for Ab and Eb, C was added to them; and lastly, the triads of Eb, G, Bb—and Bb, D, F.

The following is a Table of Beats of the Consonances in 1″, tuned according to the Doctor's directions.

Notes.	Vibrations in 1″.	Lengths of String.	$\frac{5}{6}$ 3	$\frac{4}{5}$ III.	$\frac{3}{4}$ 4	$\frac{2}{3}$ V	$\frac{5}{8}$ 6	$\frac{3}{5}$ VI.
C	480	50000	0	57·6	24·0	0	0	57·6
B	460·8	52083	64·8	0	0	0	84·4	0
A*	432	55556	32·0	0	0	0	0	0
A	409·6	58594	57·6	0	18·4	0	36·8	25·6
G*	384	62500	0	0	0	0	0	24·0
G	360	66667	0	43·2	0	0	0	43·2
F*	345·6	69445	25·6	0	0	12·8	64·8	0
F	324	74074	24·0	18·4	0	12·0	32·0	0
E	307·2	78125	43·2	0	0	0	57 6	0
D*	288	83333	0	0	0	0	0	0
D	270	88889	0	32·4	0	*9·2	0	32·4
C*	256	93750	0	16·0	12·8	0	0	16·0
C	240	100000	0	28·0	12·0	0	0	28·8

Dr. C. is one of the great number of musicians who have declared a preference for the *equal* temperament. And indeed, if we had no better *unequal* temperament than this, I think there would be no disagreement of opinion about them. It is strange that the Doctor should write in this manner: the "small interval called a comma is *about* as 80 to 81." p. 132.

June 15, 1812. A.

VI. *Description of a temporary Rick to secure Corn in Sheaves in the Fields till quite dry; also Clover, Peas, and Beans. By* William Jones, *Esq. of Foxdown-Hill, near Wellington, Somersetshire**.

Sir, The very unusual quantity of rain that fell during the months of August and September 1809, with scarcely two days of dry weather following, in this neighbourhood, put farmers to the necessity of having recourse to various modes of preserving their corn; and as I understand the Society of Arts has offered a gold medal for the cheapest and best mode of harvesting corn, and also for making hay in wet weather, superior to any hitherto practised, I beg leave to communicate some experiments I made last summer, and the result of them. In the first place, I put some wheat in small round ricks, or wind-rows, made in the common way of this county; but afterwards recollecting that the uncommon wetness of the ground might render the under part damp, I thought it prudent to examine them, (about ten days after they were set up,) and found my apprehensions so well founded, that I had the whole spread abroad; and have no doubt that, if they had remained a little longer, the corn would have been materially injured, not the bottom only, for it had contracted dampness a great way up the ricks, insomuch that I turned my attention to devise some better mode of preserving my barley in case the weather continued so rainy, as it afterwards proved. I had observed in some wet seasons before this, that many of our farmers, not being able to get their barley dry enough to put into a large rick, had set up narrow ricks, containing the produce of an acre or two, each in different parts of the same field it was grown, for the sake of expedition; and though some straw was put under them, yet the bottom contracted a great degree of dampness, so as to occasion it to smell old, and the clover was killed where these ricks had stood. My object was to prevent both these injuries; and it occurred to me, that four gate-hurdles would answer both purposes, by setting the two outside ones perpendicular, and two middle ones inclining against and supporting each other. These hurdles are usually eight feet long; the two heads in which the four bars are mortised have pointed

* From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce*, for 1811.——The silver medal of the Society was voted to Mr. Jones for this communication.

ends

ends of about a foot and a half long; the two outside ones are to be forced into the ground nearly to their full length, so that the middle brace may rest on the ground to afford some support, and the two middle ones about six inches to keep them steady. The foot of the second hurdle should be set two feet from the foot of the first, the third three feet from the second, and the fourth two feet from the third, making seven feet, and occupying a space of seven feet by eight, for barley or oats; but wheat being longer in the straw, requires the distance to be wider, viz. three feet from the first to the second, three feet from the second to the third, and three feet from the third to the fourth, which will be nine feet by eight.

It will be proper to put seven or eight small stakes, (a little bigger than a man's thumb,) from the second bar of the first hurdle to the second bar of the second hurdle, and from the second bar of the third to the second bar of the fourth, to support the sheaves from the ground, to admit air under and prevent injury to the growing clover; or small poles may be used extending from one outside hurdle to the other. The appearance of the ends of the hurdles will be as in the engraved plans, Plate I. fig. 1, and section Plate II. fig. 1, which show where the small stakes are to be placed to prevent the sheaves touching the ground; for there will be but a slight pressure on them, since the ground ends of the sheaves are to be put against the hurdle AB, and the ears of the corn a little elevated to rest against the hurdles CD, and the ears a little elevated against the centre hurdle CD; so that the ears of the corn will be all within side, and have the benefit of the air between C and D. It is to be observed that, the hurdles CD being but six inches in the ground, and the hurdles AB nearly eighteen inches, the two former will be a little higher than the two latter; which is necessary for two reasons, one is, that the higher these are, the higher the air is admitted to the middle of the rick, and the more they elevate the tops of the sheaves in the middle, for the ground ends should be lowest to shoot off the rain. But as it will be found that after two or three rows are placed around the tops of the hurdles, (for the ricks should be circular,) that the ground ends of the sheaves being largest, the tops will become nearly level, when it will be necessary to put four sheaves as at GG in the middle horizontally, forming a square, opening in the centre, which will admit air from the top of the middle hurdles CD, through this space to the middle of the rick, as the ears of each sheaf are just to meet only in the middle resting on these four sheaves*, which will give such an elevation to the tops of them, that the ground ends will be sufficiently inclining downwards to shoot off any rain that may fall. In forming the roof, the sheaves are of course to be put further in every time they are put around, till the roof terminates in a point, when two sheaves with the tops downwards spread abroad and bound with a straw band, will secure it from a great deal of rain; but if the corn is to remain out long, a little reed or thatch may soon be put on each rick.

Fearing I might not have been sufficiently explicit in describing this plan, it has occurred to me that it would be better to send a model, containing 100 sheaves, made to a scale of an inch to a foot, as to the length of the hurdles, the distance from each other, and the size of the sheaves, also to exemplify every particular of it.

The weather being so rainy for some days after my barley was cut, with every appearance of more rain, I determined, on having a few hours intermission of rain, to get the middle of the field, which was a little more dry than the rest, and to put it in small ricks, containing more than the produce of an acre, on these hurdles in the same field; it was in such a damp state as to be totally spoiled in a common rick, but was taken from these ricks into a barn in the month of January last, perfectly dry, the straw much better than could have been expected, the grain good, having been proved to grow well; for having some doubt on account of being put together so damp, I had it first tried by putting a few grains in a cloth into the earth, and have since sown it, and no other this spring, and I never had a better prospect of a good crop. The remaining part of the barley that was left on the ground, was not taken in till ten days afterwards, the grain much grown, a great deal wasted by frequently turning, and the straw spoiled.

I flatter myself it will be admitted that in wet seasons, or when harvest is so late that, as the days decrease the dews increase, and of course remain so long that there are but few hours in a day for drying, even if there should be no rain, that this method will afford perfect security to corn that is cut dry, and put up in this manner immediately from the scythe or sickle, because, if there should be grass in it, the ground end of every sheaf will be without side,

* If the corn should be very damp, and the rick made high, four other sheaves may be put higher up to convey a greater circulation of air, and operate as a bond to connect the sheaves in the middle, so that they cannot possibly slide outwards.

exposed

exposed to the sun and air to dry; and as for the grain, no part of it can get damp, because the ears but just meet in the middle, through which the air passes from the bottom to the top sufficiently to dry it. I have mentioned sheaves, because in this country barley and oats are generally bound as well as wheat; but both the former may be placed in these ricks without binding, as I had some barley put in one of them (by way of experiment), and think it to be the better mode when there is much grass in it, by carefully keeping the ears together when carried to the hurdles, where a man is ready to put it up to another on the top, and to place the ears inwards; and it is done in as short a time as the like quantity is put on a waggon, with this advantage, that whereas a waggon with three or four horses go over the clover to the great injury of it in wet weather, by this method the corn is carried by women or children in their arms to the hurdles, without the least injury to the clover; a consideration fully adequate to a little extra expense, if any, besides that of being more expeditiously secured; for every practical farmer will be sensible in how short a time an acre of corn may be carried from the circumference of an acre to its centre. As to the time of fixing these hurdles, I have ascertained that two people can fix them in five minutes, and one rick would contain the produce of two acres of barley or oats. The other advantages, besides the corn being thus sooner secured, are, that no more attendance on it is required, so that a farmer's attention may be better directed to his other harvest concerns, and that one or two of these ricks at a time (as may be convenient) may be taken into a barn to thrash, whereas a part of a large rick cannot be taken in without the trouble and expense of thatching the remainder, and be subject to the risk of rain before it may be covered again.

I trust it will be seen that by this plan there must be a great saving of the quantity as well as preservation of the quality of grain, which is known oftentimes to shed a great deal by being frequently turned to get dry. Before I thought on this expedient (last barley harvest), I am clear that a field of pease of mine required to be turned so often, that more shed out than were sown; and a farmer in this neighbourhood had a good crop of eight acres of vetches reduced to sixty bushels, by so frequently turning them for three weeks, without getting them dry at last; whereas an acre or two might have been taken up in this way a few days after they were cut, and the seed would have got sufficiently hard; but the greater part of these were so soft as to be much bruised in thrashing, and it was to be feared a great part of them would not vegetate. I had an opportunity of knowing the quantity, having the tithe of them, and proving the injury by the loss of my crop in sowing them, in so much that the land has been since ploughed.

Although I have not tried it, yet I think it is not to be doubted, but that this mode may be applied with equal advantage to clover hay, and clover seed, before it may be dry enough to put into a large rick, by being placed in this situation to dry without being so frequently turned as to deprive the hay of its finest parts, and subject the seed to great waste. In cases also when meadow hay may be dry enough to put in large cocks on the appearance of rain, how much injury do they receive by the bottom being rendered so wet as to occasion a dampness some way up, and require much time to throw abroad to dry? Whereas, in the same state of dryness, how many of such cocks may be put on four hurdles? and the bottom instead of being wet and injured will be perfectly dry, having air circulating under it, and from the two middle hurdles quite to the top; if a sheaf of reed was to be drawn up through it, as the hay got higher, a bundle of straw on top would secure it from rain, or instead of a reed-sheaf drawn up, a couple of small faggots of wood, or three or four poles bound together and placed horizontally about the middle of the rick, to admit air at each end, and render it dry enough to be carried on to a rick without furthur trouble or risk.

Hay is known to receive injury, not only from rain, but even from fervent sunshine, when nearly dry, if not frequently turned, as may be observed by the change of colour and loss of smell, which many farmers in this neighbourhood experienced in the summer of 1809, for want of hands to turn it sufficiently. I have seen a decoction of such hay made in a tea-pot, and compared with a decoction of the like quantity of good hay in another; the former was very deficient both in colour and taste to the latter, and the quality of it, of course, much deteriorated.

We know that straw, particularly of barley or oats, will be much injured by being long on the ground exposed to soaking dews, and perhaps alternate rain and sunshine, and may it not, when protected from them by this mode, be far superior for cattle to what we are at present aware of? Besides the advantages of grain, hay, and straw, being thus better preserved, and less expense of labour than by repeatedly turning in rainy seasons, there is another advantage of no small consequence, that the crops may be removed,

and

and put on hurdles in another field, (without any hindrance to sheep feeding therein) when the land from whence they were taken may be immediately ploughed; for instance, after pease, to facilitate a better fallow, (than if delayed,) to be succeeded by wheat, and ploughing clover lays for wheat, and also preparing land for turnips after vetches, to accelerate the sowing; in which case, the delay of a few days has frequently occasioned a total loss of the crop.

It is an essential consideration, that the expense attending improvements, should not counterbalance their utility: and I flatter myself, there can be no objection to this mode on that score, because gate hurdles are useful appendages to a farm, in any county, for other purposes, when not used on this occasion, and in this and other counties are requisite for dividing turnips for sheep; and as to expedition, which is of great importance in harvest concerns, four of these hurdles (as I have already observed) may be fixed in five minutes.

If, therefore, the Society for the Encouragement of Arts, Manufactures and Commerce, instituted for the laudable purposes which it professes, should think my plan combines utility with cheapness and expedition, I should consider myself flattered by their approbation, and feel a degree of satisfaction in the reflection, that I have not turned my thoughts in vain to a subject which must be allowed to be of great importance.

I am, sir,

Your most obedient servant,

Foxdown Hill, June 7, 1810. W. Jones.

To C. Taylor, M.D. Sec.

Sir,—I have been favoured with your letter, acknowledging your having received my model of a temporary rick, and recommending me to send certificates of its use.

I have to add, that the barley I had put on these hurdles last year, was done in my presence, by the same man who removed it afterwards to the barn, thrashed and sowed it; he is ready to attest my former statement, of the hurdles requiring only five minutes time to fix in the ground, of the barley preserved by them growing perfectly well, with a prospect, from its present appearance, of yielding a good crop; and with this further remark, that it was so damp when put upon the hurdles, that he was apprehensive it would be spoiled, and was much surprised, when he took it into the barn, to find it so perfectly dry.

I notice your query, whether these hurdles could not be applied to the purpose of temporary hovels for sheep, in wet weather? I think that if two of them were fixed eight feet apart, and two others placed on the top of them, covered with straw, reed, rushes, heath or furze, they would form a covered hovel of eight feet square, and afford great protection to sheep in wet weather, (particularly just after being shorn,) and ewes in the lambing season also, if some that were the most forward with lamb were selected and put into inclosures, where one end of each hurdle might be put against a hedge, or against a wall, or end of a hovel. These hurdles, covered in like manner also, would be useful, if a number of them, proportioned to the quantity of sheep, were put in the form of a square, in any part of a field, in hot weather, to afford shade. They would induce the sheep to lie there, and answer the purpose of folding, as they could easily be moved to such part of the field as wanted improvement, and be more at ease than when creeping under hedges, to the no small detriment of their wool.

I have to report to the Society, that I have this harvest made use of the hurdles on a larger scale, viz. to keep raking wheat separate from the sheaf, and which was too damp to put in sheaf; and also in small ricks of wheat for seed, to save the trouble of taking it from a larger rick, before the whole was wanted to be thrashed; and for my tithe wheat, that was not sufficiently dry to put into a barn.

I had also five acres of white pease, which were drilled where a crop of vetches had failed, so late as the 12th of May; they proved to be a very great crop, but they ripened so late, and the tops of the haulm were so green, from having shot out to an extraordinary length, that they were not all carried till the 27th of last month. At one time I almost despaired of ever getting them dry, owing to the heavy dews which fell during the night, and continued during most of the day, so as to afford but a few hours to dry my crop. I therefore took up six waggon loads from the middle of the field, on the 25th of last month, and put them on twelve gate hurdles adjoining each other, for the purpose of making one roof, and set the hurdles in the manner of my ricks. The first two loads were put on four of these hurdles at one end, which would contain four loads if necessary; the next two on the adjoining four hurdles; and the other two loads on the four remaining hurdles; so that though these three ricks were close to each other, yet being

being set up separately, they admitted air between each, from the bottom to the top, and yet adjoined sufficiently to make one continued roof to be thatched together.

When these six loads were removed from the field, I had room to turn the remaining parcels towards each other, and more towards the middle of the field, so as to have more air to dry. But they were not sufficiently dry till the 27th, when they were carried to another set of sixteen hurdles ready to take them, and each waggon load laid over the whole length of 16 hurdles, not being so damp as to require being carried up in separate ricks, as the former six loads. Some of these pease have been already thrashed, and prove to be in very good condition, as also the haulm, which is perfectly dry and sweet for cattle.

One of these ricks of pease, and probably some of the ricks of wheat, will not be taken in till the month of February next: they may therefore be inspected by any member of the Society who may visit this neighbourhood.

I have inclosed a certificate from Mr. Waldron, a gentleman of this parish, who farms his own estate; and another certificate from Mr. Hewett, also of this parish, who is esteemed a respectable and intelligent farmer: he rents a farm from Mr. Ware, brother to Mr. Ware of the house of Ware, Bruce and Co. London.

I am, sir,

Your most obedient setvant,

Foxdown Hill, Oct. 30, 1810. W. JONES.

To C. Taylor, M.D. Sec.

Certificates.

This is to certify, that I have seen some barley on Mr. Jones's farm at Culway, in the parish of Wellington, set upon gate hurdles in the harvest of 1809, which appeared to me to be a very good method to secure grain in wet seasons, or when the dews remain so long on the ground, that it cannot be got sufficiently dry to put together on other ricks. I have also seen a field of barley at Mr. Jones's this year, adjoining to my farm, sown with some of the barley put on those hurdles, which grows well, and is likely to prove a good crop; and I think this invention of public utility. HENRY WALDRON.

This is to certify, that I have seen since this harvest, on Mr. Jones's farm at Foxdown Hill, in the parish of Wellington, three ricks of wheat, and two ricks of pease, set upon gate hurdles, which appears to me a very good method

thod to secure grain in wet seasons, or when the dews remain long on the ground; that I have attentively examined those ricks, and consider it to be a public benefit that a farmer may have recourse to so cheap and expeditious a method of securing his corn in difficult seasons; whereby he is able to put it together sooner, and of course expose it less to the weather.

Wellington, Oct. 30, 1810. JAMES HEWETT.

SIR,—AGREEABLY to your request, I lose no time to give you the information you desire, respecting the temporary corn rick, and the size they may be made. The space between the two outside hurdles contains about sixty sheaves on each side, or one hundred and twenty in the whole, to reach the top of the hurdles.

Every round of sheaves afterwards takes forty sheaves or upwards, say fifteen rounds high, which makes six hundred sheaves, and which will raise the rick about eight feet from the tops of the hurdles.

It will require about seventy sheaves from the top of the above fifteen rounds, to the top of the conic roof.

Four sheaves crossing each other, five times in the centre of the rick, will form in the whole twenty, making as follows:

120 sheaves to the top of the hurdles.
600 sheaves from the tops of the hurdles to the commencement of the roof.
72 sheaves in the conical roofs.
20 sheaves in the cross or bonds of the rick.

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812 sheaves, or upwards of 81 shocks in each rick, which is more than the average produce of an acre.

The wheat in this part of the country is reaped near to the ground, and my sheaves this year are about 4½ feet long, for which the distance of 3 feet 2½ inches between the outer hurdles, and 3 feet between the inner hurdles, is calculated. The distance should be regulated by the length of the sheaves of barley and oats. When shorter than four feet, the rick should be oblong instead of round.

Faggots of wood, placed at intervals within the rick, will be found particularly useful, where pease, vetches, clover, hay seeds and meadow hay, are put into these ricks, as the faggots will promote a greater circulation of air.

The number of the cross sheaves should be according to the dampness or dryness of the corn, either in every row, or every second or third row.

Reference

Reference to Plate II. *Fig.* 1, *the Section of Mr.* JONES'S *Temporary Corn Rick.*

The letters describing the same parts of the construction of the rick, agree with those in Plate I.

AB, being the two upright outside hurdles.

CD, the two inclined hurdles.

EE, the poles or sticks on which the sheaves are to be first placed on commencing the rick, and which cross the hurdles.

HHHH, the sheaves composing the body of the stack.

II, the conical roof, the lower part of which projects sufficiently over the body of the rick, to cover it from wet; and in this roof, each round of sheaves are to be placed so as to cover the ears of the sheaves below, and gradually rise to nearly a point, over which a bundle, containing two or three sheaves, with the butt ends upwards, and tied together, cover the centre or uppermost point of the rick.

VII. *On the Combinations of Sulphur and Phosphorus with Platina*. By* EDMUND DAVY, *Esq. Chemical Operator and Superintendant of the Mineralogical Collection in the Royal Institution. Communicated by the Author.*

1. *Introduction.*

PLATINA, since its first introduction into Europe, has engaged the attention of the most enlightened chemists. The early experiments of Lewis, Marggraff, Bergman, &c. made us acquainted with many of its properties. In their attempts however to combine it with sulphur, they were unsuccessful; nor has this combination been hitherto found in nature, or been produced by any artificial processes. The crude ore of platina has been ably investigated; but the combinations of this metal with inflammable bodies, with oxygene, and with acids, have not yet been examined with precision. An accurate investigation of these compounds is wanting to complete the chemical history of platina, which, when supplied, can scarcely fail of useful applications to the arts and manufactures.

2. *Of the Combinations of Platina with Sulphur.*

The early attempts to combine sulphur with platina,

* These compounds were made in the autumn of last year; but owing to circumstances which it is unnecessary here to mention, their publication has been delayed.

were

were founded on the analogy, that most of the metallic sulphurets could be formed by the direct union of their elements. Sulphur and platina were mixed and heated together in the atmosphere, or sulphur was projected on the metal previously heated to redness; but by these methods no chemical union of the substances took place.

On resuming the subject, there appeared to me to be two modes which had not hitherto been tried; namely, that of heating the ammonia-muriate of platina in contact with sulphur; and that of exposing the metal and sulphur to an elevated temperature in an exhausted tube. Both these methods seemed favourable to combination. In one case, the metal was in a very minute state of division, and its neutro-saline ingredient volatile at a moderate heat; in the other, the volatility of the sulphur would be counteracted, and the materials exposed to a degree of heat which was impracticable in the atmosphere. On making these experiments, I had the satisfaction to find that, in both instances, sulphurets of platina were formed.

As these compounds contain different proportions of sulphur, and possess characteristic properties which will be enumerated in the sequel, they will be distinguished by the addition of the words *super* and *sub* to their common names; the former indicating the greater, the latter, the lesser proportion of the inflammable principle: these distinctions have been already adopted by chemists.

To avoid unnecessary details, it may be proper in this place to state, that the platina employed in all my experiments was procured by decomposing the ammonia-muriate of this metal at a red heat in a platina crucible. The ammonia-muriate of platina used, was obtained by treating a strong acid solution of purified platina with muriate of ammonia; the yellow precipitate, after being washed with distilled water, was dried for some days on a sand-bath at a temperature varying from 200° to about 340° Fahrenheit, and contained 44·5 grains of platina per cent.

3. *Of Super-Sulphuret of Platina.*

This compound was obtained by heating in a glass retort over mercury, a mixture of the ammonia-muriate of platina with about two-thirds of its weight of flowers of sulphur. The retort was gradually heated to a dull red by means of a spirit-lamp, and the process was continued for a short time after the gas ceased to be produced: a quantity of muriatic acid gas with some nitrogene came over, a little muriate of ammonia, with the excess of sulphur sublimed,

limed, and the super-sulphuret of platina remained at the bottom of the retort.

Physical and Chemical Properties.

Super-sulphuret of platina is of a dark iron-gray colour approaching to black. It is procured in fine powder, or in small lumps the particles of which are loosely aggregated together. In the former state, it has a dull earthy appearance; in the latter, it exhibits a feeble metallic lustre. It is soft to the touch, and when rubbed on the fingers, or on paper, it leaves a shining black mark, similar to that of black lead. It has no smell nor taste. It is not easy to ascertain its specific gravity with precision. From a trial I made, it appeared to be about 3·5, distilled water being 1; but this is probably too low. It is a nonconductor of electricity. It does not appear to be affected by the atmosphere, or by water; at least, no visible change was perceived in it after being exposed to these agents for some days. I have not been able to fuse it.

The mineral acids scarcely produce any effect on super-sulphuret of platina. A single grain of it was boiled successively in pure concentrated nitric, nitro-muriatic, and sulphuric acids, but it apparently underwent no change; the residuum, after being washed and dried, lost no sensible weight. In the case of the nitro-muriatic acid, however, there was a minute quantity of sulphur acidified, which was indicated by a slight precipitate on the addition of nitrate of barytes.

A strong solution of pure potash when boiled with super-sulphuret of platina occasioned no visible change in it; but when the alkali was dry and fused with it, a partial decomposition took place. When it is mixed with oxy-muriate of potash and heated, it is decomposed with brilliant combustion. Sulphureous acid gas and oxygene gas are expelled, and the odour of chlorine is exhaled. The same mixture detonates loudly when it is smartly struck with a hammer.

When super-sulphuret of platina is mixed with fine zinc filings and exposed to an elevated temperature in a retort over mercury, it is decomposed with vivid ignition, and sulphuret of zinc is formed. When heated in a similar way with iron filings, it is partially decomposed. When it is heated in chlorine gas, sulphurane appears to be formed; but the decomposition seems to be only partial: the results I have not examined with precision.

Super-sulphuret of platina is not decomposed at a red heat

heat in vessels excluded from the air; but in the atmosphere it undergoes partial decomposition at a moderate heat, the sulphur burns with a blue flame, and sulphureous acid gas is formed: at a dull red it is completely decomposed, and the platina remains in a state of purity.

Analysis.

The mineral acids could not be employed to ascertain the composition of super-sulphuret of platina. It might be analysed by the agency of zinc filings and the acids; but it was unnecessary to resort to complicated processes, when methods more simple, and susceptible of greater accuracy, could be adopted. As it was readily decomposed by heat, and the quantity of metal in a given weight of the ammonia-muriate of platina easily ascertained, its composition could be determined both by analysis and synthesis.

From two experiments made in both of these methods with considerable precaution, its component parts are deduced.

First Experiment. 10 grains of super-sulphuret of platina decomposed by heat in a platina crucible afforded 7·17 grains of pure platina.

Second Experiment. 8 grains of super-sulphuret decomposed by heat on a piece of Florence flask, furnished 5·75 grains of platina.

From the first of these experiments 100 grains of super-sulphuret of platina contain

Platina	71·7
Sulphur	28·3
	100·0

For 10 : 7·17 : : 100 : 71·7.

From the second, 100 grains contain

Platina	71·87
Sulphur	28·13
	100·00

For 8 : 5·75 : : 100 : 71·87.

In the two synthetical experiments 10 grains of the ammonia-muriate of platina, heated with sulphur, furnished in one case 6·14 grains, and in the other 6·1 grains of super-sulphuret.

Now 10 grains of the ammonia-muriate of platina contained 4·45 grains of platina. Hence these experiments indicate rather less sulphur than the preceding ones; but the difference is trifling, and is easily accounted for. In collecting

collecting the results of these last experiments from the retorts, there must have been a slight loss, which is not noticed in the statements. Without regarding the fractional parts, I venture to state the composistion of 100 parts of super-sulphuret of platina as

Platina.....	72
Sulphur....	28
	100

4. *Of Sub-Sulphuret of Platina.*

This substance was procured by heating platina in contact with sulphur in an exhausted tube. The tube employed was about four inches long, and 1¼ inch in diameter; at the open end it was made small for the convenience of being easily drawn off after exhaustion. The materials were mixed and introduced nearly in equal weights, a stop-cock was then cemented to the tube, it was exhausted, hermetically sealed, and heated for about ten minutes in an open fire; towards the end of the process, the bottom of the tube was heated nearly to redness, to expel the excess of sulphur; the sub-sulphuret of platina alone remained at the bottom of the tube.

Physical and Chemical Properties.

Sub-sulphuret of platina is of a dull blueish-gray colour. It is obtained in the form of powder, or in small pieces the particles of which are loosely coherent. Its lustre is earthy; but when rubbed on the fingers, or on paper, it leaves a shining mark of a blueish-gray colour, having the metallic lustre. It is rather harsh to the feel. It has no smell or taste. Its specific gravity is about 6·2, that of distilled water being 1. It is a nonconductor of electricity. Like the super-sulphuret of platina, it does not appear to be affected by exposure to the atmosphere, or in water. Its chemical properties closely resemble those of the super-sulphuret. The mineral acids scarcely affect it at a boiling heat. It is decomposed when heated with oxymuriate of potash or zinc filings.

Analysis.

The only simple and accurate method of determining the composition of sub-sulphuret of platina appeared to me to be by the agency of heat, and to this mode I had recourse. In the direct union of its elements, there was always a loss arising from the circumstance that sulphur remains fluid at

high

high temperatures in exhausted tubes; and from its being in a constant state of motion, particles of the sub sulphuret formed, were occasionally deposited on the sides of the tube with a little sulphur, and they could not easily be collected.

In two different experiments carefully made, 5 grains of sub-sulphuret decomposed at a low red heat, in a platina crucible, afforded 4·2 grains of platina, and 5 : 4·2 : : 100 : 84.

Hence 100 parts of sub-sulphuret of platina contain

Platina	84
Sulphur . . .	16
	100

The sub-sulphuret of platina, whilst it possesses some properties analogous to those of the super-sulphuret, differs from it in colour, lustre, specific gravity, and proportion of sulphur; properties sufficient to establish its existence as a distinct combination.

5. *Of the Combinations of Platina with Phosphorus.*

Chemists have hitherto paid but little attention to the combinations of metals with phosphorus. The papers of M. Pelletier appear to contain most of what is known concerning these compounds. He first combined phosphorus with platina* by exposing the metal, phosphoric glass, and charcoal powder to a strong heat; and by projecting bits of phosphorus on platina heated to redness. From the experiments I have made, I cannot but regard the methods of M. Pelletier as very imperfect. Platina and phosphorus combine with great facility, at a heat considerably below redness; at a red heat, a portion of the phosphorus is expelled, and at still more elevated temperatures the combination is nearly destroyed.

Phosphorus, like sulphur, appears to combine in two different proportions with platina. The same methods employed in forming the sulphurets were successfully adopted in making the phosphurets of platina, and they will be marked by the same distinctions.

6. *Of Super-Phosphuret of Platina.*

This compound was procured by heating together the ammonia-muriate of platina with about two-thirds of its weight of phosphorus in small bits, in a retort over mercury. Towards the end of the experiment, the retort was heated

* *Annales de Chimie*, tome i. p. 100, and tome xiii. p. 105.

heated to a dull red for a few minutes to expel every thing volatile. A quantity of muriatic acid gas having the odour of phosphorus and some nitrogene gas came over, a little muriate of ammonia with the excess of phosphorus sublimed, and super-phosphuret of platina was formed.

Physical and Chemical Properties.

The physical and chemical properties of super-phosphuret of platina resemble those of the super-sulphuret. Its colour is iron black, or very dark gray. It is in fine powder, or in small pieces in a loose state of aggregation. When in this last state it exhibits a dull metallic lustre. It marks the fingers or paper, but the lustre it gives is much inferior to that of the super-sulphuret. Its specific gravity is about 5·28, that of distilled water being 1. It has no taste or smell. It did not appear to be affected by being exposed to the atmosphere or water for some days. It is a non-conductor of electricity. When it is heated on a thin slip of platina below redness, it becomes an ignited mass and diminishes in bulk, but its colour scarcely varies. On raising the heat to whiteness by means of a blow-pipe, it fuses, and perforates the platina with holes. It is but little affected by the mineral acids at any temperatures. When it is heated with fine zinc or iron filings, it undergoes little change. It is partially decomposed with vivid ignition when heated with oxymuriate of potash, and oxygene gas and chlorine gas are evolved. When mixed with the same salt, and struck smartly with a hammer, it detonates with a loud report. It is decomposed when heated in chlorine gas, phosphorane is formed, and a combination of chlorine and platina remains; but I have not yet examined these results with precision.

Composition.

It was not easy to find unexceptionable methods of analysing the super-phosphuret of platina. The mineral acids had no apparent action on it. Though oxymuriate of potash decomposed it, the process was tedious, and the results by no means satisfactory. At a high temperature long continued, it was decompounded; but little dependence could be put in this mode of operating, as in cases when Hessian crucibles were used there was almost always a considerable loss, and platina ones could not be employed without insuring their destruction.

The only method which seemed to offer satisfactory re-

 sults,

sults, was that employed to ascertain the composition of the super-sulphuret by synthesis, and to this I had recourse.

From several experiments two of the most accurate are selected, from which its composition is deduced.

First Experiment. 30 grains of the ammonia-muriate of platina were heated with phosphorus in a small green glass retort coated with clay, for about half an hour, during which time the retort had been heated to a dull red. The super-phosphuret collected when the retort was cold, weighed 18·95 grains. Now these 30 grains of the metallic salt contained 13·35 grains of platina; consequently the 18·95 grains of super-phosphuret consisted of 13·35 grains of platina + 5·6 grains of phosphorus. This experiment indicates 30 per cent. of phosphorus in the super-phosphuret.

Second Experiment. 10 grains of the ammonia-muriate of platina heated with phosphorus precisely in the same way as the last experiment furnished 6·34 grains of super-phosphuret. This experiment very nearly agrees with the preceding one. And 100 parts of super-phosphuret of platina may be regarded as composed of

Platina....	70
Phosphorus	30
	100

7. *Of Sub-Phosphuret of Platina.*

This substance was obtained by heating together platina and phosphorus in an exhausted tube, similar to that used for procuring the sub-sulphuret of platina. The intensity of their mutual attraction is beautifully demonstrated by this experiment. At a temperature considerably below redness, they combine with vivid ignition and flame, and unless the tubes are strong they are very liable to be destroyed.

Physical and Chemical Properties.

Sub-phosphuret of platina is of a lead gray, or blueish-gray colour. It is either obtained in small porous masses, or (in cases when as much as 20 grains of the metal are employed) in imperfectly fused and crystallized pieces. The crystals are small cubes. When it has undergone fusion its lustre is little inferior to that of lead, but in a porous state it has little brilliancy.

Its specific gravity when in porous pieces is about 6. After fusion there can be no doubt but it must be considerably more. It has no taste or smell. It is a non-conductor of electricity. When it is heated to a strong red on a slip

of

of platina, the odour of phosphorus is exhaled, and its surface assumes a darker tint. When the heat is urged to whiteness it combines with the platina, which it perforates with holes. It is partially decomposed with ignition when heated with oxymuriate of potash. When it is heated in chlorine gas, the results are similar to those obtained in the case of the super-phosphuret.

Composition.

The difficulties which were opposed to the direct analysis of the super-phosphuret, equally apply to the sub-phosphuret. Its composition was ascertained by the direct union of its elements; and though only comparatively small quantities of the materials could be employed with safety, yet from the manner of operating, it would seem that the results are susceptible of considerable accuracy.

From several experiments I have selected two of the most accurate, from which its composition is determined.

First Experiment. 10 grains of platina heated with phosphorus in an exhausted tube for about seven minutes furnished 12·1 grains of sub-phosphuret.

From this experiment 100 grains contain

Platina	82·64
Phosphorus	17·36
	100·00

Second Experiment. 20 grains of platina heated with phosphorus in an exhausted tube, precisely as in the other experiment, afforded 24·3 grains of sub-phosphuret. This experiment, which I regard as the more accurate of the two, gives the component parts of sub-phosphuret of platina as

Platina	82·3
Phosphorus	17·7
	100·0

The difference between these experiments is not considerable. From a comparison of them with others, I shall venture to consider the mean as the nearest approximation. Hence 100 parts will be composed of

Platina	82·5
Phosphorus	17·5
	100·0

General Observations, &c.

On the received opinions concerning the composition of

the

the ammonia-muriate of platina, it is possible to question the accuracy of the preceding statements relative to the super-sulphuret and super-phosphuret of platina; they may perhaps be considered by some persons as compounds of oxide of platina and the respective inflammable principles. From the experiments I have made on this metallic salt, I consider it as a compound of platina, chlorine, ammonia and water. But should the facts I have to relate be considered as inconclusive with regard to this point, still, it appears to me, they can only be explained by admitting the constitution of the above combinations of platina to be such as I have stated.

When the ammonia-muriate of platina is exposed to a dull red heat in a retort over mercury, (after being dried for some time at a temperature above 212°,) it is entirely decomposed, the only products are muriatic acid gas in quantity, nitrogen gas, aqueous muriatic acid, sublimed muriate of ammonia, and pure platina. In this experiment the following changes may be presumed to take place. One portion of the ammonia is decomposed, its nitrogen is evolved, its hydrogen combines with the chlorine, to form muriatic acid gas, one part of which assumes the gaseous state, the other part is found in combination with the residue of the ammonia and with the water. This explanation appears to be strictly conformable to facts; at high temperatures, ammonia is well known to be resolved into hydrogene and nitrogene gases, and muriatic acid gas to be formed from chlorine and hydrogene gases. As the gaseous products are the same, and the general results precisely analogous, whether the ammonia-muriate of platina be decomposed alone, or in contact with sulphur or phosphorus, the same explanation of the facts will equally apply to them all.

I have found that when metallic oxides are heated with freshly sublimed muriate of ammonia, a quantity of water is formed, ammoniacal gas is evolved, and compounds of the metals and chlorine are obtained; nor are these results affected, even when the muriate previously contains water. As in the case when a mixture of red precipitate and muriate of ammonia are heated together, white precipitate appears to be the only combination of mercury formed, from which calomel may be obtained by heating it with quicklime, it may not perhaps be unworthy the consideration of the manufacturer, whether calomel and pure ammonia may not be more œconomically procured by these methods than by those modes at present adopted. I merely throw this out as a hint; it would be a happy circumstance, if any of the

the results of my inquiries should admit of immediate useful applications.

If the ammonia-muriate of platina be regarded as a compound of oxide of platina and muriate of ammonia, I do not see how the preceding facts can be explained. If its composition is such as I have stated, Sir H. Davy's theory happily applies to all the phænomena.

I have made some experiments on those substances which are considered as triple compounds of oxide of platina, acids and fixed alkali; and they all appear to me to be compounds of chlorine, platina and alkali: thus they are decomposed when heated with sulphur and phosphorus, and furnish substances which Sir H. Davy has denominated sulphurane and phosphorane; compounds precisely similar to those obtained when these inflammable bodies are heated directly in chlorine gas.

At Sir H. Davy's request, I made a number of experiments on those substances which have been considered as oxides of platina, and the evidence of facts obliges me to regard almost all the statements in our elementary books, relative to the oxides and salts of platina, as incorrect. It is unnecessary in this place to detail the experiments on which these opinions are founded; they will more properly form the subject of a distinct paper.

It is also said to be a distinguishing property of platina, that it is precipitated by sulphuretted hydrogene in the metallic state. This is not the case. It is obtained in the form of a black powder resembling the super-sulphuret in some properties, but differing from it in others; thus, it is soluble in nitro-muriatic acid, and contains a large quantity of sulphur. Sulphuret of potash likewise precipitates platina in combination with sulphur; but I have not yet satisfied myself as to the true constitution of these compounds.

In forming the combinations of platina with sulphur and phosphorus, it was not easy to obtain uniform results, owing it seems to their partial decomposition at high degrees of heat, and the difficulty of regulating the temperature with precision. The doctrine of definite proportions, which has been so amply illustrated by the researches of the most enlightened chemists, served in some measure to direct me in my experiments, and the results I have obtained appear to harmonize with this doctrine. Thus, the sub-sulphuret and sub phosphuret of platina contain one proportion, and the super-sulphuret and super-phosphuret two proportions of the inflammable principles. A tabular view of these combinations will exhibit them in a more distinct manner. They are derived from the foregoing experiments.

Grs. of Platina.	Sulphur.		Phosphorus.	
100	19·04	sub-sulphuret	21·01	sub-phosphuret
	38·8	super-sulphuret	42·85	super-phosphur.

Platina has a very strong affinity for phosphorus. This is exhibited in a remarkable manner in the energy with which they combine. I am acquainted with no simple or accurate method of analysing combinations of the metals with phosphorus. As chlorine decomposes the phosphurets of platina with facility, it seems to offer a simple mode of examining refractory compounds of this kind.

Zinc has been supposed to have but little affinity for sulphur, because chemists have not been able to combine these substances directly. In the preceding pages it is stated, that zinc filings decompose the super-sulphuret of platina with ignition. The experiments I made in the autumn of last year, lead me to conclude that the affinity of zinc for sulphur is at least equal to that of iron. The intensity of their mutual attraction is evident from the circumstance, that zinc filings and sulphur when exposed to an elevated temperature in exhausted tubes combine with vivid ignition and flame. I also found that zinc filings by the agency of heat decompose the sulphurets of lead, molybdena, copper and nickel, and sulphuret of zinc is formed.

The combinations of platina with sulphur and phosphorus will probably admit of some useful and œconomical applications. The sulphurets of platina are insoluble in all the mineral acids, and this property seems to afford an easy method of obtaining pure platina from the crude ore. For this purpose the nitro-muriatic solution should be neutralized by ammonia, the precipitate washed, dried, and heated with an excess of sulphur; sulphurets of the different metals in the crude ore would thus be obtained, and they may be all separated from the sulphuret of platina by the agency of the mineral acids. As the super-sulphuret has the property of giving to paper, &c. a considerable degree of lustre, it may perhaps be used as a pigment, in cases where permanency and lustre are required.

Platina is well known to be admirably adapted for vessels and instruments employed in œconomical or experimental processes; but its application is very limited in consequence of its high commercial value. It would be a very desirable and important object to be able to give a permanent coating of platina to kitchen utensils, steel instruments,

ments, and philosophical apparatus; to prevent the destructive effects of the atmosphere, and to preserve them from the agency of acids and other corrosive substances. I cannot but indulge a hope that these new compounds will admit of some applications of this kind. I made some attempts to plate steel, iron, copper and brass with the super-sulphuret and super-phosphuret of platina, but obtained no very satisfactory results; the coatings were in some cases very partial, and in others not permanent. It will be unnecessary to enter more into detail, as Mr. James Stodart and myself intend to resume the subject, and to make the experiments in a more refined manner.

It seems probable that the combinations of sulphur and phosphorus with platina exist in nature, and M. Proust* considers the black powder remaining after the ore of platina has been digested in nitro-muriatic acid, as containing both sulphuret and phosphuret of platina: new researches however are wanting in order to establish this point.

The phosphurets of platina described in this paper differ materially from the combination obtained by M. Pelletier, both in the proportions of phosphorus they contain, and in their sensible properties. The methods hitherto employed to form the class of metallic phosphurets appear to be very imperfect, and can scarcely furnish in any instance correct results; they are quite impracticable in some cases. Thus the phosphurets of gold and mercury can only be formed at comparatively low temperatures; and the phosphuret of gold is entirely decomposed by a moderate heat. Other instances might be mentioned in which the existing combinations are partially destroyed at more elevated temperatures, as the phosphuret of nickel and the super-phosphuret of platina. The new mode I have adopted, that of heating the metals and phosphorus in an exhausted tube hermetically sealed, seems to me to be simple and accurate, and in its application promises to furnish more correct results than have yet been obtained.

VIII. *Description of an Acorn Dibble, invented by Mr.* CHARLES WAISTELL, *of High Holborn* †.

SIR, IN consequence of information that Government

* *Annales de Chimie*, tome xxxviii. p. 171.

† From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce*, for 1811.——The Society are indebted to Mr. Waistell for this improvement. One of these instruments is preserved in the Society's Repository.

 wanted

wanted intelligence respecting the best mode of dibbling acorns, I have made an improvement on the acorn dibble in the Society's Repository, which I presume will answer well the desired purpose. I therefore send herewith a drawing of it, requesting you will have the goodness to lay it before the Society of Arts, &c.

Thorn bushes and thickets are the natural guardians of young oaks from the depredations of cattle of all kinds, on forests and other grounds on which they pasture. By means of this implement, acorns may be deposited in the interior of bushes, as well as in open grounds, with rapidity and accuracy. And presuming that such an implement would be of great utility to many individuals, and also to Government, I wish much to have it made known as generally as possible among those who are most likely to profit by it; and which I think may be best effected by the Society of Arts, &c. giving an engraving of it in their next volume; provided they concur with me in thinking it may be the means of rearing an increased number of oaks; to promote which every possible facility should be given.

Permit me on this occasion to observe, that many proprietors of landed property are not sufficiently aware that a greater or less proportion of almost every estate would, if judiciously planted, pay the proprietor much more than the rent it could be let for to a farmer. It would, therefore, give me great pleasure to see in the Society's volumes more communications from successful planters. I trust there are numerous persons of this description who want only to be reminded, how greatly they might benefit individuals, as well as their country, by publishing, or communicating to you, such well ascertained facts of their success as planters, as they may be in possession of; and in order to direct their attention to the nature of the information that is chiefly wanted, I beg leave to refer them to pages 80 and 81 of the Society's 27th volume, wherein numerous particulars respecting the planting, management and produce of woods are enumerated.

I am, sir,

Your obedient servant,

No. 99, High Holborn, June 12, 1811. CHARLES WAISTELL.

To C. Taylor, M.D. Sec.

Reference

Reference to the Engravings and Section of Mr. WAISTELL'S *Improvement of the Dibble for planting Acorns.* Plate II. Fig. 2, 3, and 4.

a represents the handle of the dibble, which dibble is a rod $\frac{3}{4}$ of an inch in diameter, moveable in the tube of a stave, which stave is externally about two inches diameter. *b* a tin or metal tube fixed on the exterior part of the stave, and of the same bore or aperture of the tube of the stave: when a hole is made in the earth by the point of the dibble *d*, the acorn is dropped down the metal tube, and on drawing up the dibble by its handle to the height of the letter *e*, the acorn *c* passes through a large opening into the dibble tube, and from thence falls into the hole made by the point of the dibble in the earth, when by moving backwards and forwards the cross handles *g g*, fixed on the top of the hollow stave, the soil surrounding the hole in the earth is loosened by the iron wings *f f* and deposited on the acorn. Fig. 4, *h* shows a section of the iron wings *f f* belonging to the bottom of the hollow stave.

Supposing that you wish to plant an acorn in the middle of any bush, you are to press the instrument through it into the ground, make a hole in the earth by the point of the dibble rod, then raise the rod above the hole where the two tubes communicate, drop the acorn down the tube *b*, which falls immediately through it and the lower part of the stave-tube into the hole previously made by the rod, which hole is instantly covered by the soil raised by the wings. The dibble rod may be occasionally passed down the metal tube, to be certain of its being perfectly clear.

IX. *Examination of a Species of Plumbago from Africa. By* EDMUND DAVY, *Esq. Chemical Operator and Superintendant of the Mineralogical Collection in the Royal Institution. Communicated by the Author.*

THIS substance was brought from Mozambique, on the continent of Africa, where it is said to occupy a considerable extent of surface. I am not aware that it has been described by mineralogical writers; I shall therefore notice its physical and chemical properties.

Its colour is iron-gray. It is disseminated in small laminæ, in a loosely aggregated matrix, composed of feldspar, quartz, and mica. The laminæ intersect each other in different directions, but exhibit no appearance of crystallization. Its lustre is metallic, like that of polished steel. It is is soft and unctuous to the feel, and strongly marks the fingers or paper, like common plumbago. It is a conductor of electricity. The specific gravity of the pure substance I could not ascertain, as it was intimately blended with its matrix; but when separated as much as possible from earthy particles, it was 1·6, distilled water being 1. I have not been able to learn its geological situation, or the nature of the rocks with which it is associated; but from the circumstance of its occupying distinct strata, and from its specific characters, it probably resulted from the degradation of the primary granite, and might be classed among that order of rocks termed secondary. If regard were only paid to its external characters, this substance would probably find a place among the ores of molybdena; but the effects of chemical agents on it proved that it could not belong to this class of bodies. It was unaltered before the blow-pipe on platina, charcoal, and borax. It was scarcely affected by any of the mineral acids. A little of it was boiled successively in concentrated test nitric, muriatic, nitro-muriatic, and sulphuric acids; but its lustre remained unimpaired, and it lost no perceptible weight. On examination, however, these acids were found to have taken up a minute quantity of iron, which was indicated by a slight blue precipitate on the addition of prussiate of potash.

When the ore was exposed to the agency of pure potash, or to nitre at a red heat, it appeared to be unaffected, and in the first trials with these substances no satisfactory results were obtained. Decided evidences of its composition were gained by heating it to redness in contact with arsenic acid, an experiment which I found had been previously made by the illustrious Scheele *, who seems to have considered the carbonic acid not as a product, but as a constituent part of plumbago.

Analysis.

I was necessarily confined to the use of very small portions of the ore, not only on account of the limited quantity in my possession, but likewise in consequence of its highly refractory nature. In no instance was I able to consume so much as five grains in close vessels, so as to obtain decisive results. From several experiments I have selected three of the most accurate, and from them I shall venture to state its composition.

First Experiment. $7\frac{1}{2}$ grains of the ore (carefully separated from the matrix) were mixed with 30 grains of arsenic

* *Mémoires de Chimie de M. Scheele,* tome ii. p. 31.

acid

acid and exposed to a strong red heat for about 25 minutes over mercury, in a small green glass retort coated with clay. Nine cubical inches of carbonic* acid gas were obtained, besides a quantity of oxygen, and white crystallized oxide of arsenic sublimed. After the residuum had been well washed with distilled water, and dried at a low red heat, it weighed $4\frac{1}{4}$ grains, and appeared to be unaltered in its general characters. A half of a grain of earthy matter was now separated from the $4\frac{1}{4}$ grains, which, on examination, was found to be silex, alumine, and a minute quantity of iron. Now, from the accurate experiments of Messrs. Allen and Pepys†, 100 cubical inches of carbonic acid gas contain 28·60 grains of carbon. And 9 cubical inches contain 2·57 grains. Hence these $7\frac{1}{2}$ grains of the ore furnished

furnished	4·25	grains of ore unaffected
	2·57	carbon
	0·5	silex, alumine and iron
	0·18	loss
	7·50	

Second Experiment. 5 grains of the ore were heated with 35 grains of oxymuriate of potash, in a similar manner as the preceding experiment. 6 cubical inches of carbonic acid gas were obtained, and a quantity of oxygene. The residuum carefully collected was treated with pure muriatic acid, and digested for some minutes at about 70° Fahrenheit. The acid solution was then passed through a filter, and the solid matter collected, washed, dried, and heated to redness. It weighed 3·1 grains, and had all the appearance of the native ore, except that its lustre was very slightly impaired. A few minute particles of silex were disseminated in it. The acid solution gave only a slight indication of iron with prussiate of potash, and no ascertainable quantity of earthy matter separated, when it was neutralized with carbonate of potash.

Hence these 5 grains of the ore afforded with oxymuriate of potash,

6 cubic in. carbonic acid gas =	1·71	carbon
	3·1	ore scarcely affected
	·19	silex with a little iron.
	5·0	

* The quantity of carbonic acid gas in these experiments was determined by the absorption with lime water, and a strong solution of caustic potash.

† Phil. Trans. vol. xcvii. p. 290.

With

With a view to determine the quantity of iron the ore contains, the following experiment was made.

Third Experiment. 8 grains of the ore were mixed with about 70 grains of oxymuriate of potash (previously fused) and heated in a platina crucible. At a dull red heat the mixture became an ignited mass: the heat was increased until all the oxygene was expelled from the salt. The ore was not much affected by this treatment. About 150 grains of nitre were now introduced, and the heat raised to a strong red: after half an hour the crucible was examined, a quantity of the ore still remained. A fresh portion of nitre was added, and the heat raised and continued for about two hours, during which time fresh portions of nitre were occasionally introduced.

The crucible was now examined, and exhibited to the eye no remains of the ore. The solid matter was treated with diluted test muriatic acid, and after being digested for some time at a moderate heat, it wholly dissolved except a quarter of a grain, which proved to be principally silex with a little dust from the charcoal. The acid solution was treated with a slight excess of ammonia, and after a short time a whitish flocculent precipitate appeared. It was collected on a filter, washed, and heated to a strong red in a platina crucible. It hardened in the fire, and had now the characters of alumine coloured by iron. It was bruised to powder and digested in diluted test muriatic acid; the greater part of it remained insoluble, and was principally alumine with a little silex. The acid solution was now treated with prussiate of potash, and after some time the prussiate of iron was collected on a filter, washed and dried. As no ascertainable quantity of it could be separated from the paper, the filter was burnt in a platina crucible, and a red heat continued until all the carbonaceous matter was consumed. The oxide of iron obtained, weighed a quarter of a grain, and was partly attracted by the magnet.

This experiment affords only an imperfect approximation as to the real quantity of iron in the ore. If the iron be considered as existing in the ore in the metallic state, it can scarcely be estimated at more than two or three per cent.

The preceding experiments furnished distinct evidences as to the nature of this substance, that it is a species of plumbago, and consists almost entirely of carbonaceous matter. In its general characters, it seems to bear a striking resemblance to the artificial products of some iron furnaces in the East Indies, well known by the name of Kish iron,

iron. I am not acquainted with any varieties of plumbago which contain so small a quantity of iron, or which exhibit such refractory properties. Those examined by Scheele, Pelletier, and other able chemists, furnished more iron, and differ in other respects from the subject of this paper.

X. *Mr.* Bakewell *in Reply to Mr.* Farey's *Geological Observations.*

To Mr. Tilloch.

Sir, In the last number of The Philosophical Magazine you have inserted some strictures of Mr. J. Farey on what he is pleased to call an erroneous statement in my Lectures, respecting a map of England, which I asserted to be, *as far as I knew*, the first attempt to present in a distinct view "a geological outline of the arrangement of the most important mineral substances in South Britain." I believe this was what I stated in my lectures, and notwithstanding Mr. Farey's two pages on the subject I repeat the assertion again.

The week previous to the publication of Mr. Farey's letter, Mr. Greenough favoured me with the inspection of his map, which I had not before seen, or even heard of. With respect to what Mr. Farey or Mr. Smith may have done towards producing a Geological Map of England I am still unacquainted; nor am I very desirous to learn, if the imaginary "*zigzag fault*" and "*great Derbyshire fault*," in the former gentleman's Survey of Derbyshire, be specimens of what we may expect from his geological labours. Mr. Farey has asserted that the metalliferous limestone in Derbyshire is the very lowest stratum of rock in England; on the contrary, I believe that the same limestone, which disappears in the north-west side of Derbyshire, rises again in the district of Craven in Yorkshire, and rests upon slate. As I have visited this district several times, and Mr. Farey, from his own acknowledgement to me, has never seen it; I presume that my evidence on the subject may be entitled to as much weight as the doubts of Mr. Farey, who has nothing to advance in their support, but the existence of his imaginary great fault, for which I conceive there is no proof whatever. The limestone of Craven is as similar to that of Derbyshire, as the different beds in that county are to each other; it contains the same metallic ores, and rises from under a coarse mill-

stone

stone grit and shale grit near Burnsal, on the river Wharf, in the same manner as the metalliferous limestone of Derbyshire. All the ablest geologists that I have conversed with, who have examined the lime rocks of both counties, are convinced of their similarity; and had Mr. Farey visited the country without any prejudices from his particular theory, I am satisfied he would have formed the same conclusions.

As Mr. Farey has written so much and so frequently on what he calls a great Derbyshire Fault, which extends from the south side of Nottingham to Ashburn in Derbyshire, and from thence in a circuitous path to Macclesfield in Cheshire, and further to the north, where he ceases to trace it; I am sure geologists will be obliged to him to inform them where he has actually found this fault 'in situ' in any part from Nottingham to Ashburn? what is its exact breadth? whether it be merely a slip, or whether it be filled with mineral matter? and of what kind? He will also inform them how the limestone has passed over or under this fault, so as to appear again with veins of lead ore at Breedon and in other situations on the south side of the Trent. It will scarcely satisfy the curious to be told that these lime rocks are mere anomalous masses, though it may be an easy method of dismissing a difficulty which destroys the very existence of Mr. Farey's Derbyshire fault. I am inclined to believe that Mr. Farey has written so much in sober seriousness respecting this great phænomenon, that he earnestly believes in its reality; but I presume that he has the singular satisfaction of enjoying this opinion undivided. I never have met with any one resident near its supposed course, or who has paid attention to the subject, who believed that it had any other existence than in the mind of the discoverer, who has given to it a local habitation and a name. With respect to the existence of what Mr. F. calls the Zigzag Fault, the most intelligent coal workers that I have conversed with who have pits in the vicinity are not disposed to admit its existence. The western side of Derbyshire adjoining Cheshire and part of Staffordshire is much broken by faults; but I have seen no proof offered that any one of these faults extends over a considerable tract of country. On the southern side from Nottingham to Derby, the very existence of such a fault as Mr. Farey describes has no where been proved in any part that I am acquainted with. The loose sand rock and gravel of Nottinghamshire and Derbyshire appears to have been thrown over the surface in extensive patches which

which terminate upon the red marle on the southern side; but there are no indications of the surface having been fractured along the line where the termination of the sand rock is observed. I therefore repeat again my request, that Mr. Farey would distinctly point out the situations where this great fault, on which he has written so much, can be seen and proved to exist. A particular fracture at any one part, will not be sufficient to prove the identity of a fault in any other part, without further evidence than the conjectures of the observer.

The assumption of imaginary facts in geology has a tendency to retard the progress of the science more than any other cause. The application of conjectures to explain the operation of causes which we have not been able to prove or trace, may be allowed where such conjectures are kept distinct from facts; but when they are confounded with them, and reasoned upon with as much solemn gravity as if they were real entities, they "darken council by words without knowledge."

At the conclusion of Mr. Farey's letter, he attributes to me the adoption of Mr. Whitehurst's opinion that the toad-stone of Derbyshire has been injected in a state of fusion between the beds of limestone. This opinion I have most distinctly disavowed; though I am inclined to believe that the basaltic amygdaloid of that county has been the product of submarine volcanoes thrown over the lime at different periods of time when the whole of that country was under the surface of the ocean. This was offered merely as a conjecture, to which no importance was attached.

I am, sir,

Your obedient servant,

July 14, 1812.

R. Bakewell.

P.S.—It is with some reluctance that I have been compelled to enter the lists of controversy with Mr. Farey; but as I am at this time examining for the Earl of Moira a part of Charnwood Forest, I take the opportunity of observing that Mr. Farey's statement, that there is no appearance of stratification in the slate rocks of this district, is erroneous. All these rocks which I have examined are composed of grau wacce, roof slate, horn-stone slate, and porphyritic slate, which are most distinctly stratified, but in an opposite direction to the slaty cleavage of the stone. On the minera'ogy of this district I shall probably offer some remarks to the public.

XI. *Dr.*

XI. *Dr.* Lambe *on Arsenic.*

To Mr. Tilloch.

Sir, The experiments of which I herewith transmit you an account, form a part of a supplement to my experiments on arsenic, read before the Royal Society this spring. I wished them to be read before the same body this season: but having been disappointed in that expectation, and having mentioned the principal fact to several persons, I wish the following short account to be inserted in The Philosophical Magazine.

I am, sir, &c.

2, King's Road, Bedford Row, July 17, 1812. William Lambe.

On the Decomposition of White Oxide of Arsenic by Lime. By William Lambe, *M.D.*

I prepared some lime by exposing pieces of marble in a crucible to the heat of a smith's forge, making it so caustic, that no effervescence took place by the affusion of diluted muriatic acid, though a few minute bubbles were still formed. To effect this, I found it necessary to keep the marble in this strong heat two complete hours. I mixed three ounces of this lime with its weight of white oxide of arsenic, which I had previously sublimed and pulverized. The mixture was introduced into a coated glass retort, to which a tube was joined, the end of which was plunged into water in a pneumatic trough; and the retort was gradually made red hot. The water at first rose into the tube, showing an absorption of gas by the matter in the retort; but when the retort became red-hot, its neck became covered, internally, with moisture, and a cubic inch or two of gas was expelled, which was not soluble in any notable proportion in water. After this, the water rose again into the tube, and undulated for some time, up and down, with a strong vibratory motion. Finally, three or four cubic inches of gas were expelled, by far the greater part of which proved to be carbonic acid.

On breaking the retort (which was done as soon as it was cool) the lime was found to be converted into carbonate of lime; being completely neutralized, and effervescing strongly with an acid.

It is readily demonstrable, that the carbonic acid and water produced in this experiment are formed at the expense of the arsenic. Some of the oxide is retained by the lime,

lime, and some more (but blackened) is sublimed into the neck of the retort; but, upon the whole, there is a considerable loss of weight of the arsenic. But the more minute details of the experiment, with some other phænomena connected with it, I must reserve till I can bring before the public the memoir of which it forms a part.

I must here observe, that for the success of this experiment, it is essential that it be performed in close vessels. It will not succeed in an open crucible. Three times I have failed when using glass retorts. Some carbonic acid was expelled from the tube as before, but mixed with a large proportion of insoluble gas; and little or no carbonate of lime was found; but a new compound, with the nature of which I am not at present perfectly acquainted. In each these experiments, it appeared that a hole had been formed in the retort, and the difference of the result must be attributed, therefore, to the admission of atmospheric air.

When I used common lime, which was very caustic, I found the production of carbonic acid, apparently, much more abundant. From three ounces of this lime, and an equal weight of white arsenic, I procured 90 cubic inches of carbonic acid, and the lime was likewise converted into carbonate of lime.

Finally, I have once used lime which had been exposed to so intense a heat that the crucible was partly melted. This lime put into an acid did not emit the smallest bubble of gas. Heated, in close vessels, with its weight of white oxide of arsenic, some carbonic acid was produced: but the matter in the retort did not prove to be carbonate of lime; but another compound with the nature of which I am at present no further acquainted, than that it dissolves in muriatic acid without effervescence, and forms a crystallizable salt.

XII. *On certain Points connected with the Super-position of the Strata of England.* By DAVID MUSHET, *Esq.*

To Mr. Tilloch.

SIR, THE subject of stratification has lately been so ably treated in your valuable Magazine by Mr. John Farey, that it cannot fail to excite a very great interest among those interested in the success of geological and philosophical pursuits. With a patriotism truly laudable, Mr. Farey invites his countrymen to the study of the subject at home, as as sufficiently rich in itself to afford ample compensation to the most unwearied investigation. I am induced to trouble you with this letter, in consequence of a call made on your readers by Mr. John Farey, in your Number for February, for information on some particular points connected with the super-position of the strata, as well as from some remarks said to have been made on a similar subject by Mr. Robert Bakewell in your Number for March.

Mr. Farey, I am afraid, must have mislaid his notes, made along with me in the Forest of Dean eighteen months ago, when I detailed to him the regular succession of the upper coal series from the upper red down to the yellow lime; at the same time offering to show him in several places the yellow limestone emerging from under its massy incumbent, associated in situations where the thickness of the covering stratum could have been nearly ascertained, or even subjected to accurate measurement. Mr. Farey however says, in page 103, that he has never been able, satisfactorily, to ascertain which was the incumbent measure or stratum to the yellow, or, as it has been termed by some since the able analysis of Mr. Smithson Tennant, the magnesian lime. In an arrangement I have made for my own use, I term it the second limestone, reckoning upwards and beginning with the Derbyshire series.

It may be proper to observe, that I conceive the superstratum to the yellow lime to be that which is denominated by Jameson the old red formation, and in my arrangement, the *great red*, in contradistinction particularly to the marly red, which surmounts the upper coal series. The thickness of this stratum is variable. Where its immense beds are allowed without the intervention of faults or waves to stretch themselves out in regular succession, I have not estimated it at less than 1600 yards, and in some places, particularly from the high grounds to the south of Ross in Herefordshire to its termination or regular ending at Berristone Hill between Ross and Ledbury, not less than 2000 yards.

This vast stratum is composed of alternations of particoloured stone, in which the red colour predominates; breccia, regularly stratified clay, clay more friable and in a marly state. The joints of the rocks afford gypsum. The beds of clay, rock salt, or salt springs, as at Droitwich, Nantwich, &c.: the marl, sometimes stratified gypsum. It appears to me extremely probable, that the gypsum pits described by Mr. Farey, page 104, had been worked in a lower bed of the great red, and immediately over the yellow lime. This, however, ought not to be admitted as proof, unless the

the fact were established by other circumstances, since the whole of the great red, and frequently the third limestone series above it, forms deep red soil, in which gypsum is frequently found.

Ascending the series, the great red may universally be considered as the next stratum in the order, and one of its most distinguishing characters is the plum-pudding-stone, or breccia, which occupies a superior situation in the mass, forming bold edges, and stretching into table land, where the stratification has become more horizontal. It is an opinion not unfrequently advanced by geologists, that fragments of breccia are a certain indication of an approach to a primitive country. I should rather infer, that such fragments betokened an approach to the upper beds of the great red. What gives considerable strength to the former opinion is the fact of the most mountainous districts, hitherto considered primitive, being contained in the superficies occupied by this grand boundary to our two great coal series. How different is the fact where elevation of surface continues! On these extensive plains of breccia, where the measures are fairly extended, rests the third, or great limestone series. This is the limestone which underlays the Staffordshire, Shropshire, South Wales, and Forest of Dean coals. It is also the same as the Bristol limestone, which is seen on the banks of the Avon, rising rapidly from under the coal measures. This rock is also various in its thickness: in the Forest of Dean I estimate it at 200 yards, and from the enlarged dimensions of its beds on the west side of the great Welsh bason, its thickness must be from 250 to 300 yards.

Immediately upon this rock rests from forty to sixty yards of what by many would be termed alluvial sandstone. It is easily distinguishable from every other sandstone by its pure siliceous base. It also contains from 15 to 20 yards of breccia in solid stratified blocks, and several veins of the same, in a more disintegrated state, which open and shut in the same way as metallic veins. How irreconcileable to these facts are the opinions of those who determine the class of the stratum, whether it be alluvial or not, by the absence or presence of a single pebble! These regularly stratified beds of breccia, and veins of the same, rest under more than 50 veins of coal, and nearly two perpendicular miles of higher strata. The upper bed of this peculiar sandstone series partakes in some places of the nature of limestone, and abounds with vegetable impressions. In Wales it is called the Farewell Rock, because it terminates the coal and iron stone measures, reckoning downwards, in some places, as in the Forest of Dean, the lowest coal is only separated from it by a few inches of clay.

The measures which rest upon the third limestone I denominate the upper coal series: the boldest feature in this assemblage is a gray greenish micaceous sandstone, measuring in thickness fully 200 yards; this forms uncommonly bold high land, from Myrthr Tidvil to Caerphilly, along a considerable extent of the South Wales coast, and in the Forest of Dean, fine plains with rounded edges, from 7 to 800 feet above the level of the sea. In Wales it is called the Gray Rocks, and the Pennard Rock in the neighbourhood of Bristol.

Between this and the Farewell Rock are contained the iron-making coals and ironstone of the South Wales Bason, the former of which, according to Mr. Martin, amounts to 95 feet of workable coal. I have not seen any statement, nor have I heard of any coal being worked in South Wales at any great height in this series above the Gray Rock. Should this prove the case, the Forest of Dean supplies the void that would otherwise have been occasioned in the general section, as it contains from 3 to 400 yards of coal measures, occupying a higher range in the series, and immediately above the great Gray or Pennard Rock. These measures are composed of twelve seams of coal, and are surmounted by a considerable thickness of a straw-coloured sandstone, containing occasionally beds of red marl. From this circumstance, and from the absence of red ground upon the surface of the coal measures in the Forest of Dean, and in South Wales, as far as I have seen, I am induced to think that this sandstone, containing the red marl, where there is cover, is overlaid by the upper Somersetshire coals, and these again, by cliffe or shale, on which rest the upper red, which I conceive to be a species of red calcareous marl. Subject to this arrangement, we find that the upper series is formed into three distinct divisions with red marl above, and the great red (from under the third limestone below); the whole thickness of which series may be estimated at 1200 yards.

Having thus, according to my observation, supplied the chasm complained of by my friend Mr. Farey, I shall shortly subjoin an outline of what I believe to be the general order of the British strata.

London clays and sands reaching to the chalk:

Measures from the chalk to the third or great oelite, comprising the green sand, Bedford sand, 1st oelite or Portland

Portland stone, clunch, or Oxford clay; 2nd oelite, fuller's-earth, &c. &c. &c.

Measures from the great oelite to the upper or marly red, containing a great thickness of blue clay, the blue lias, and white lias limestone, &c.

First division of the great upper coal series, comprising 5 or 6 small coals which have been sunk to, and got under the lias limestone at Duncarton, Camberton, Timsborough, &c. &c. Somersetshire.

Second division of the upper series, as now worked to a considerable extent in the Forest of Dean, above the Great Gray or Pennard Rock.

Third division of the upper coal series, between the Great Gray Rock and the third limestone series; comprising the coals worked for iron making, and for other purposes, along the great Welsh coal basin. Those worked at Kingswood and Ashton, Nailsea and Bakewell, near Bristol, and in the neighbourhood of Coleford in Dean Forest.

The third limestone series:

Great red.

Yellow, or magnesian limestone.

Derbyshire and Yorkshire coal series:

The four great sandstones, containing the stinking coals, and forming very high land and precipitate terminations along a considerable extent of Derbyshire and Yorkshire.

The great or limestone shale.

Derbyshire limestone series, comprising 1st, 2nd, 3rd, 4th limestone alternating with toadstone.

I shall now beg leave to remark what is said to be stated by Mr. R. Bakewell, in his Lectures; namely, that the Derbyshire fourth limestone is seen in Yorkshire, resting on slate. Could this fact be established, it would be interesting in the extreme. But when this gentleman appeals to similar instances in Shropshire and in Wales, I am afraid he refers to one of those numerous formations in the great red, commonly denominated primitive. Examples of this sort are to be found in the neighbourhood of Charnwood Forest, Mount Sorrel, the Malvern hills, &c. I agree with him as to the fact of limestone covering slate, and this slate resting upon granite; but I differ from him by assigning to the granite a different place in the strata. I am not yet satisfied that the granite is the foundation of the series, but in many cases only an accidental formation in the great red, sometimes stratified and sometimes assuming the form of an irregular crystallization: so that, in my estimation, the fourth Derbyshire limestone is not that which is generally found resting upon slate; but more probably the yellow lime: this being the concomitant stratum to the great red, is frequently subjected to those mighty derangements peculiar to this immense mass. In proof of this I refer your readers to an examination of the country in this neighbourhood, from Flanly Abbey, where the yellow lime just emerges from under the great red, not as a regular ending or basset, but in consequence of a powerful lift or swell in the measures along Blassden and May Hills, passing behind Newent to the west, sometimes wheeling, sometimes advancing, forming gentle helmet-topped hills covered with brushwood, till acquiring a more uniform and contiguous ridge, it makes directly to Ledbury, and subsequently the angle of its rise becoming much elevated it overlays the western side of the Malvern hills to a considerable height.

This limestone, as it is seen in the neighbourhood of Ledbury and Malvern hills, has been accurately described by Mr. Horner in the Geological Transactions. That part of the limestone series which he denominates argillaceous slate, I conceive to be an inferior sort of limestone, as there are few of the beds but what yield nodules of limestone. In Kirkby in Derbyshire it wears exactly the same appearance when exposed to the weather. In some districts it is called mudstone, and is soluble in water, so as to make sinking through it difficult. As a proof that the limestone now described is the same with the yellow or magnesian, which forms the coping stratum of the Derbyshire and Yorkshire coal fields, I have to state the fact of the Derbyshire thick first coal having been worked many years ago, from under the crop of the yellow lime, near the town of Newent in Gloucestershire. Appearances had been so favourable in the crop as to induce the proprietors about ten or twelve years ago to sink a shaft, between 80 and 90 yards deep, and put up a powerful engine. This pit was sunk in the lower beds or mudstone of the yellow lime. The coal was found six feet thick, but of an inferior quality; the roof also, from the numerous joints in the mudstone, was so heavy as to break timber two feet in diameter. Before abandoning the work, those concerned had, by cutting a drift across the rising plane of the measures, ascertained the existence of the lower coals, which, upon comparing with the Derbyshire section, I found exactly to correspond with the small coals found under the Greasly Sheanich and Alfreton coals. This curious circumstance was further corroborated to me

at

at the time, by finding fragments of well-known argillaceous ironstone amongst the spoil at the pit ground.

I remain, &c.

Coleford, Gloucestershire, DAVID MUSHET.
20th April, 1812.

XIII. *On the Elements for reducing the Polar Distances of the principal fixed Stars which pass near the Zenith of the Royal Observatory at Greenwich. Taken from Dr.* BRADLEY'S *Catalogue, by Mr.* FIRMINGER.

Charles-street, Somers Town,
July 25, 1812.

SINCE the time that the reductions of the latitude and longitude of the stations used in carrying on the trigonometrical survey of this country have been in part published, many gentlemen engaged in the pursuit of practical astronomy have either made use of them in settling the position of their observatories, or have employed themselves in the verification of these results with a series of astronomical observations made at the different stations or objects that have been given in the survey. Some of these verifications were attempted with sextants; but as the sextant itself cannot be depended on generally, to an accuracy nearer than 20 or 30 seconds, this combined with the errors arising from the artificial horizon used with it render such observations, in deductions where an uncertainty of only a few seconds is to be looked for, of little or no value, even though a mean, from a great number of repetitions, should be taken. The usual method which astronomers have adopted to determine the difference in latitude between two places, not very distant from each other, has been by observing the zenith distances at the two places of a number of stars which pass near the zenith of each place; these, when properly reduced, give the difference of latitude between the two places, or the number of degrees in the contained arc. The instruments generally employed in such operations have been those denoted by the name of Zenith Sectors, and they are the best instruments when well constructed for this purpose, admitting of a more accurate measure, from being divided on an arc of a much larger radius than can be used in the construction of instruments for other astronomicial observations. The zenith sector in the Royal Observatory at Greenwich is divided upon an arc of twelve feet radius; its principal use there is for determining the error in collimation of the two mural quadrants; and this has been done from a series of observations upon the meridional

meridional zenith distances of γ Draconis, observed at the same time both with the sector and quadrants. The zenith sector used by Lieut.-colonel Mudge in the trigonometrical survey is divided on an arc of eight feet radius. And from the credited ability of the maker of this latter instrument, and the progressive state of improvement in the construction of astronomical instruments at the period it was finished, a much greater degree of consistency might have been expected, than what is found amongst the deductions of the observations made with it, that have been already published. The number of observations made at each station, however, must tend to render the final result almost free from error, and the amplitude of the celestial arc as correct as perhaps it can be obtained. The observations made with this instrument for determining the length of a degree of the arc of the meridian have tended to show that the plumb-line with which the instrument was adjusted, is deflected from the perpendicular, from local attractions upon the earth's surface, and that consequently the observed measure of the celestial arc contained between the zenith of two places, is not their true difference in latitude. Over a small country like that of England, the difference of latitudes of places may be determined with almost any degree of accuracy by a trigonometrical survey; and it is presumed that the general survey which has been carrying on for several years past, under the direction of the Ordnance, has not been deficient in this respect, as the persons to whom the operations have been intrusted are provided with the best instruments perhaps that from the state of modern improvements could have been contrived for the purpose. Although zenith sectors are undoubtedly the most accurate instruments for determining zenith distances, a very considerable degree of accuracy may be obtained by small instruments of an appropriate construction, as meridian circles, repeating circles, and even the circle by reflection when properly mounted upon a stand, the inaccuracy being continually lessened by a succession of observations either upon the same star, or upon a number of stars, so as at last to become only the fraction of a second.

If a series of astronomical observations on the distances of several stars from the zenith were accurately made at different stations, or at such objects as were bisected in the survey, and whose situations have been correctly reduced, results of considerable interest might be deduced, as the degree of reflection of the plumb-line at each station might be determined.

In

In the year 1802, whilst Lieut.-colonel Mudge was employed in observing the zenith distances of the stars at the Royal Observatory at Greenwich, with his zenith sector, I computed for him the elements for the reduction of the principal stars which pass near the zenith contained in Dr. Bradley's Catalogue, and published in the Nautical Almanac of the year 1777; and as they may, perhaps, be useful to astronomers, or to such gentlemen as are desirous to put into practice the verifications above alluded to, I have arranged them in a Table. The first column contains the Right Ascension, which is not computed with great accuracy, being only intended to give the time of the star's passing the meridian. The second column contains the number for aberration in North Polar Distance; the third column its Maximum; the fourth column the Logarithm of this Maximum; and the fifth the Annual Precession in North Polar Distance computed to three places of figures. The formulæ by which the number and maximum of aberration were computed are as follow:

Put $P =$ the angle of position.
$Z =$ an arc.
$M =$ the maximum.
$N =$ the number for aberration in north polar distance.

Log. $s, P =$ log. c. Æ + log. s, obl. ecliptic + log. secant latitude.

Log. $t, Z =$ log. cosec latitude + log. tang. P.

Log. $M =$ 1·30103 + log. cosec Z + log. s, P; and

Right Ascension, or Longitude being in Quadrant.	Latitude.	P being Acute.	P being Obtuse.	
1 4	N	Long. ✳ $-3^s - Z$	Long. ✳ $+3^s + Z$	$-N=$ The sun's longitude when the aberration is greatest negative, or the star is furthest north
1 4	S	Long. ✳ $-3^s + Z$	Long. ✳ $-3^s - Z$	
2 3	N	Long. ✳ $-3^s - Z$	Long. ✳ $+3^s - Z$	
2 3	S	Long. ✳ $+3^s - Z$	Long. ✳ $-3^s + Z$	

The aberration becomes $- M \times c$ (☉'s long. ∽ N.)

Example from the Table.

Suppose it was required to find the aberration of β Draconis on August 10, 1809.

☉'s Æ August 10th, noon	9^h	19^m	14^s
Æ β Draconis	17	25	58
Time of β Draconis passing the meridian nearly	8	6	44

For which time the sun's longitude is	$4^s\ 17°\ 40'$		
Number for aberration . .	5 23 3		
	1 5 23	cos	9·91132
	Log. maximum		1·25739
	Aberration = 15″,80		1·19871

Hence the aberration of β Draconis on August 10, 1809, was 15″,80, which is additive to the mean place to get the apparent, as the cos 1° 5′ 23″ and *M* are both negative.

The effect of deviation may be computed from the general Tables of Deviation I gave in the Philosophical Magazine of last month, which, together with the Annual Precession, will give the whole reduction, except a small quantity arising from the semi-annual solar nutation, which is too small, except in very nice observations, to be considered.

	Right Ascension to beg. year 1802.			No. for Aberration in N. P. D.			Maximum of Aberration in N. P. D.	Log. Maximum of Aberration in N. P. D.	Ann. Precession in N. P. D. to beg. year 1802.
	H.	M.	S.	S.	°	′	″		″
α Cassiopeæ -	0	29	21	8	21	0	16,541	1·21853	−19,886
θ Cassiopeæ -	0	59	7	8	26	47	15,688	1·19554	−19,387
θ Persei - -	2	30	42	9	14	39	12,231	1·08745	−15,870
τ Persei - -	2	40	17	9	0	53	12,823	1·10803	−15,343
γ Persei - -	2	50	32	9	23	22	12,750	1·10552	−14,751
α Persei - -	3	10	15	9	26	14	11,378	1·05609	−18,530
δ Persei - -	3	28	53	10	0	8	10,380	1·01631	−12,284
λ Persei - -	3	51	53	10	9	45	10,536	1·02267	−10,634
7 Camelopardalis	4	41	27	10	29	24	10,604	1·02526	− 6,739
Capella - -	5	2	5	11	3	27	8,021	0·90424	− 5,013
δ Aurigæ - -	5	42	33	11	23	6	10,224	1·01146	− 1,525
19 Lyncis - -	7	6	41	1	4	17	10,975	1·04429	+ 5,751
26 Lyncis - -	7	40	16	1	11	51	9,448	0·91582	+ 8,494
i Ursæ Majoris -	8	45	38	1	3	54	11,189	1·04878	+13,262
θ Ursæ Majoris -	9	19	38	2	9	30	12,972	1·11299	+15,339
β Ursæ Majoris -	10	49	48	2	28	39	16,071	1·20614	+19,117
γ Ursæ Majoris -	11	43	21	3	12	8	16,670	1·22193	+19,997
ζ Ursæ Majoris -	13	15	55	4	0	57	18,277	1·26188	+18,961
η Ursæ Majoris -	13	39	45	4	8	20	17,793	1·25022	+18,181
υ Herculis - -	14	18	29	5	6	33	18,460	1·26623	+10,279
μ Draconis - -	17	4	7	5	18	22	19,536	1·29082	+ 4,840
β Draconis - -	17	25	58	5	23	3	19,357	1·28739	+ 2,966
κ Cygni - -	19	15	30	6	14	53	19,368	1·28705	− 6,238
i Cygni - -	19	24	42	6	17	7	19,180	1,28281	− 7,244
θ Cygni - -	19	31	8	6	18	8	19,000	1·27876	− 7,764
c Draconis -	20	0	48	6	8	8	19,601	1.29224	− 3,378
ν Draconis - -	17	52	1	5	28	24	19,341	1·28648	+ 0,700
2 π Cygni - -	21	39	29	7	12	52	17,982	1·25483	−16,398
7 Lacertæ - -	22	23	9	7	18	58	17,796	1·25028	−18,287

XIV. *Proceedings of Learned Societies.*

WERNERIAN NATURAL HISTORY SOCIETY.

At the meeting on the 28th of March, Professor Jameson read an account of a flœtz gypsum formation, which occurs on the banks of the White-adder, near Kelso. Likewise of a beautiful flœtz quartz found in beds in the coal districts of Fifeshire; and of the occurrence of basalt, amygdaloid, and trap-tuff, in a coal-formation, newer than the old red sandstone, and its accompanying porphyry, but probably older than the general mass of the rocks of the newest flœtz-trap formation.—At the same meeting, Mr. Leach read a description of the Pig of Orkney and Shetland, which he inclined to consider as a distinct species. And the Secretary laid before the meeting a very full and interesting thermometrical Register and meteorological Journal to Davis Straits and back again, kept by Mr. John Aitken, surgeon.

At the meeting on the 11th of April, Dr. Macknight read a mineralogical description of Tinto, a noted mountain in Lanarkshire. It appears to be of flœtz formation; probably resting on the grey wacke which pervades the whole mountainous districts in the south of Scotland. Around the base is found conglomerate, containing rounded masses of grey-wacke, iron-clay, flinty-slate, splintery hornstone, quartz, felspar, mica, &c. Where the rock becomes finer-grained, it approaches in some places to grey-wacke, and in others to those portions of the old red sandstone formation which are conjectured to alternate with the newer members of the Transition series. Over the conglomerate masses of claystone, greenstone and greenstone passing into clinkstone and porphyry-slate successively appear, till we reach the summit, which, along with the whole of the upper part, is found to consist of compact felspar and felspar-porphyry.—The disposition of the rocks in this mountain is conformable to the idea of secondary deposition, by assuming a finer and more crystalline texture as they ascend; and the occurrence of claystone and felspar in a position corresponding to what is observed on the Eildon Hills, the Pentlands, the Ochils, Papa Stour, Dundee, and in other places, seems to favour the hypothesis of a particular overlying formation, in which these substances are prevailing ingredients, extending over a considerable portion of the lower country of Scotland.

In

In the bed of the Clyde, to the eastward of Tinto, amygdaloid appears, having nodules of calcedony coated with green earth; also calcspar, and portions of steatite.—Towards the north, the conglomerate forming the base of Tinto passes into the sandstone of which the whole inferior districts of Lanarkshire are composed. It is to the waste of this rock that we owe the splendid scenery of Cora Linn, and the other celebrated falls of the Clyde, which exhibits in its course so many charms of nature, and may indeed be said to carry along with it beauty and fertility.

At the same meeting, the Secretary communicated a very curious Meteorological Journal kept by Governor Graham during his residence in Hudson's Bay.

KIRWANIAN SOCIETY OF DUBLIN.

The proper business of the Society had been suspended in the early part of the month, on account of the illness and death of the late Mr. Kirwan, whose relation to the Society was of so particular and interesting a nature.

June 17. The reading of Mr. Donovan's paper was continued. The next hypothesis which the author examined was that given in the Encyclopædia Britannica, which professes to explain the phænomena of electricity "from the known laws by which other fluids are observed to act upon one another." It was first argued that the property of perfect mobility, although assumed, was nevertheless incompatible with the hypothesis: and some observations were made on the supposed identity of electricity, light, and elementary fire. The principle which assumes the pressure and equilibrium of the electric fluid was next examined. The causes of pressure and equilibria were considered: it was shown, that in the hypothesis, the properties necessary for producing these effects, far from being implied, are even denied to belong to the electric matter. Some phænomena of the transit of electricity through vacua, which the hypothesis professes to explain, were shown to be not the less involved in difficulty, from the unsupported nature and insufficient application of this supposed pressure. The evidence for the existence of vibrations was then questioned, and conceived to be invalid; a particular direction of them was argued to be inconceivable, from the very definition offered of a vibration. The difference between positive and negative electricity, as laid down in the hypothesis, was shown to exist in the mode of of expression rather than in effect, and the explanation given for the phænomena of attraction and repulsion was deemed inadequate. The theory of excitation was afterwards examined, and argued to be independent of diminished pressure between the cylinder and rubber, or of any reflective power of the amalgam. Concerning the charging of the Leyden phial, it was observed that the vibrations in which the charging depends, by counteracting, should destroy each other. Some arguments against the opinion that points do show the direction of the electric fluid, were then adduced; and the reading concluded by showing that notwithstanding the hypothesis disclaims the doctrine of accumulation and diminution, yet that both states must be inferred by legitimate deductions from the remaining principles of the hypothesis.

[Erratum in the proceedings of the Kirwanian Society for May, for Ecles's hypothesis *read* Eeles's, throughout.]

IMPERIAL INSTITUTE OF FRANCE.

[Continued from vol. xxxix. p. 461.]

ASTRONOMY.

Comets.—Two comets were discovered in 1811, in the South of France: they have been most minutely observed and calculated by the astronomers of Paris. The first, which was announced without any person paying the slightest attention to it, latterly excited much of the public attention, from the instant of its acquiring a tail of several degrees in length. The second has not as yet excited any great sensation, nor is it calculated to do so, because it is not yet visible except with a telescope, and because, as it is removing from the earth and the sun, it must gradually diminish until it escapes altogether from our view.

The first was discovered at Viviers on the 26th of March 1811 by M. Flaugergues, corresponding member of the Imperial Institute, who communited his first observations. The comet was then very feeble in point of light, without a tail or any apparent nucleus. This last circumstance rendered it very difficult of observation. On the 19th of April, M. Burckhardt succeeded in seeing it with the searcher of his telescope, but in the telescope itself it was invisible, because it magnified too much. We notice this fact in order to spare some trouble to the curious in future, numbers having flocked to the Imperial Observatory last October, to see the comet in our large telescopes, when they might have seen it more distinctly at home with the smallest glasses.

glasses. Obliged to have recourse to his colleagues, M. Burckhardt received from M. Bouvard such observations as the limited time would permit him to make. As soon as he had a sufficient number (this may be three, only providing they are neither too near nor too distant from each other) M. Burckhardt calculated the orbit. The smallness of the arc and the uncertainty of the observations rendered this labour very troublesome. Nevertheless this first approximation was sufficient to announce that the comet would soon disappear, because it proceeded to lose itself in the rays of the sun, *i.e.* to rise and set almost at the same time with the sun. M. Burckhardt also announced that the comet would re-appear about the middle of August; that it would then be nearly at the same distance from the earth, but twice as much nearer the sun, and that in this way its light would be quadrupled: that on the 13th of August it ought to rise an hour and a half before the sun; that on the 15th of September, the day of the perihelion passage, it would be so near the sun as to set no longer; that from this epoch it ought to augment in size and lustre for about a month, because it would then approach the earth; that it would afterwards gradually become feeble, but that we might follow it with glasses to the month of January, perhaps even a little longer: lastly, that the distance of the comet from the earth would be always considerably greater than that from the earth to the sun. These predictions, which were confirmed by the event, excited no sensation because they came too soon. M. Olbers, a distinguished astronomer, one of the deputies from Bremen, at the baptism of the King of Rome, carried with him from Paris the elements calculated by M. Burckhardt. M. Gergone of Nismes made use of them for constructing an ephemerides of the course of the comet from February 1811 to the end of March 1812. This labour, which was merely an object of curiosity, as long as the comet by its brilliancy attracted all eyes towards it, became a matter of utility when it was necessary to use telescopes to view it.

Astronomers nevertheless, after having determined so long beforehand every thing which they thought proper for interesting the public, continued to observe the comet in silence, and to compare their observations with calculation, in order to rectify the trifling but inevitable errors in the first attempt at finding an orbit. M. de Flaugergues, who first saw the comet, calculated all his observations. After having by himself determined the elements of the orbit, he thought he discovered some resemblance

blance with those of a comet observed in China 510 years ago. This remark, if it could have been verified, would give the true measurement of the revolution and of the ellipsis of the comet; but this knowledge is by its nature very uncertain, when we have no other observations than those of a single appearance. M. de Flaugergues, on going back to antiquity, found several comets, the appearances of all which differed by 510 years; and they would give a high degree of probability to his conjecture, if the indications of historians were not too vague to admit of the orbit being calculated. We have therefore nothing certain as yet on this head. Messrs. Bouvart, Gauss and Lindenau, who have also determined the orbit, think on the contrary, that the period could not be less than 1000 or 1500 years, and that it might be much more. When we have eight months observations we shall perhaps be in a little less uncertainty on this subject; but even this is very problematical. The above comet after all that has been said of it had nothing in it more remarkable than any other comet. After having determined the route which it ought to take, astronomers can only repeat what is printed in every treatise of astronomy. But they ought not to confine themselves to this dry detal: we ought to have some dissertations on the physical constitution of the comet, on the nature and cause of that long tail, which in telescopes looks like a veil attached to a woman's head, and which is symmetrically unfolded on both sides in two opposite curves, at first distant, but which end by approaching and being melted into each other. Astronomers in this respect are no further advanced than our ancestors. The explanation which Newton gave of the tails of comets, accounts on a large scale for the most remarkable phænomena, *i. e.* for the direction, which is always, with a slight variation in the prolongation of the line which unites the centres of the sun and of the comet, with a slight curvature which inclines it towards the place which the comet has quitted; but it is difficult thereby to account for the inclination of the other branch in a contrary direction; and this phænomenon has been remarked by all astronomers. Wherefore does this tail, or that atmosphere of which the tail is the prolongation, appear separated in every point from the head or nucleus?

This obscure interval, which has been remarked in a similar manner constantly, does not take place in all comets; but it is not without example. Is the separation real? Is it an optical illusion, and, if it does take place, what

what can be the cause of it? To these questions geometricians and astronomers will make no answers, because they do not know any good ones, and because they neither desire to give or to receive any other kind. In the absence of these solutions so desirable, recourse was had to the foreign Journals for the calculations which were represented as curious observations. They gravely informed us how many miles the comet travelled in a given time—vain inquiry!—which an astronomer is sometimes teased into, but to which he can never attach any real importance. The comet, at the time of its greatest rapidity of motion, never equalled that of Venus, far less that of Mercury. We see Venus almost at all seasons; she approaches the Earth much more closely than any comet; no person ever asks how many leagues she travels daily; and no person even took it into his head that she would fall upon the Earth. It must be confessed, however, to the glory of the present age, that these terrors are greatly diminished, and men of strong minds have testified their displeasure at them.

The second comet was discovered at Marseilles, on the 16th of November, by M. Pons, who had formerly been the discoverer of seven or eight others. The director of the Imperial Observatory of that city, M. Blanpain, communicated the discovery to us, at the same time transmitting the observations which he had made on the 17th, 18th, and 19th of the same month. Its movement was daily about 10′ right ascension, against the order of the signs, and 33′ declination towards the north pole. It was then very feeble, and very difficult to see at Paris: the bad weather also frustrated the attempts of our astronomers, and they experienced many obstacles in making some doubtful observations. M. Burckhardt however calculated the orbit; and although he considered it only as an awkward attempt, it was found almost entirely similar to that which M. Gauss determined from observations perhaps a little better, because they were made in a climate more to the southward. However this may be, the comet has already passed its perihelion, and will soon disappear; its smallest distance from the Sun was as 8 to 5 of the distance from the Earth to the Sun. In spite of this distance, which accounted for its dullness and the slowness of its movement, if the weather had been more favourable, it would have been easier to have observed it than the beautiful comet which we still see, because its nucleus was more apparent and better defined. We know that it does not resemble any of the 100 comets whose orbits are known.

New

New Lunar Tables.

The calculation of these two orbits, notwithstanding the difficulties with which it was attended, was a mere recreation to M. Burckhardt, which did not hinder him from putting the finishing stroke to the immense researches which he had undertaken on the subject of the moon's motions. Six years have scarcely passed, since the Institute and the *Bureau des Longitudes* lavished their encomiums on the tables of M. Burg, the Vienna astronomer. These tables, constructed on several millions of excellent observations, supported besides by the analytical inquiries of Count Laplace, and augmented by several new equations, have been generally adopted by astronomers, and nothing hitherto has warranted the least suspicion of their accuracy. M. Burckhardt's first idea therefore was not precisely to make new tables, but of a form more convenient for calculators. Mayer had remarked, that he could diminish considerably the number of the equations and arguments, by employing only the true place of the sun, and by correcting successively the arguments by the equations already calculated. This form was attended with inconveniences, which induced M. Schulze of Berlin to re-model Mayer's tables in order to bring them to the mean arguments. M. Carlini of Milan has recently announced that he has formed the project of a similar transformation for the tables of M. Burg. M. Burckhardt first entertained this idea, and on this occasion he wished to ascertain if there did not exist other equations which deserved to be entered in the lunar tables. Formerly, when an astronomer undertook new tables for any planet, he commenced them *de novo*, and risked doing them less accurately than his predecessors. By the method now adopted, we are no longer exposed to these retrograde movements: the moderns seek for the corrections of the tables most accredited, which they compare with observations: they equal the errors of these tables to a function which comprehends the corrections of the elements employed, and the new equations which they wish to introduce. In this way they determine at once the slight corrections of the elements already known, and the coefficients of the neglected equations.

In this way M Burckhardt went to work: he began by giving to M. Burg's tables the new disposition which brought them to the mean arguments, and comparing them under this new form, not only with all the observations calculated by M. Burg, but also with thousands of more recent observations, he met with several advantages in this tedious process: namely, that of subjecting to a new examination the coefficients so well discussed by M. Burg, of extracting them directly from the observations, with the changes which the mean arguments occasioned, of introducing the new equations which the observations required clearly; and yet not to lengthen the calculations, since, if on one hand it increased the number of the equations, on the other it simplified the formation of the arguments; which is an inestimable advantage, particularly for the calculators of ephemerides.

After having finished this work, M. Burckhardt subjected his tables to a new proof, by comparing them with all the passages of the moon by the meridian which could be observed in the first ten months of 1811, either by himself at the Observatory of the Military School, or by M. Bouvard, at the Imperial Observatory.

We cannot say more at present on the subject of these tables, for they have been but a very short time in our possession; but every thing inclines us to think that they will be at least as precise, and above all more convenient even than those of M. Burg, and we trust that astronomers will be gratified by their immediate publication.

[To be continued.]

XV. *Intelligence and Miscellaneous Articles.*

CAOUTCHOUC DISCOVERED IN NORTH AMERICA.

We are informed that Professor Barton has discovered the substance called *Caoutchouc*, in considerable quantity, in several vegetables growing abundantly in the vicinity of Philadelphia, and in other parts of the United States. He finds it in the berries of *Smilax caduca*, and other species of this genus; and has pursued the progressive modifications of this singular substance from its first appearance in the infant berry,—in which it occurs as a mere viscid fluid,—to its complete formation in the adult fruit, in which it shows itself in elastic plates, *already formed.*

This, it is believed, is the first instance of the discovery of *Caoutchouc* in a *formed* state in any vegetable: unless, indeed, something of the kind does take place in the *Hypochœris radicata.* The Professor intends to publish at length, in the course of a few months, his experiments and observations on the subject of this elastic matter, as the

the product of different species of *Smilax*, &c. Meanwhile it may be observed, that *Smilax cauuca* is one of the more common of the North American species of the genus; and that the matured fruit, with its *Caoutchouc*, is greedily devoured by some species of birds, especially certain species of *Tetrao*.

Has *Caoutchouc* been observed in the fruit, &c. of any of the European species of *Smilax*, or in any of the other genera belonging to the natural family of *Asparagoideæ?* This is a great genus in North and South America. The berries of that singular plant, the fœtid *Smilax herbacea*, whose flowers, both male and female, having the strong carrion smell of some *Stapeliæ*, &c. solicit flies to deposit their larvæ upon them, seem to contain no *Cautchouc*.

DESCRIPTION OF THE ERUPTION OF SOUFFRIER MOUNTAIN, ON THURSDAY NIGHT, THE 30TH OF APRIL, 1812, IN THE ISLAND OF ST. VINCENT.

The Souffrier Mountain, the most northerly of the lofty chain running through the centre of this island and the highest of the whole, as computed by the most accurate survey that has yet been taken, had for some time past indicated much disquietude; and from the extraordinary frequency and violence of earthquakes, which are calculated to have exceeded two hundred within the last year, portended some great movement or eruption. The apprehension, however, was not so immediate as to restrain curiosity, or to prevent repeated visits to the crater, which of late had been more numerous than at any former period, even up to Sunday last, the 26th of April; when some gentlemen ascended it, and remained there for some time. Nothing unusual was then remarked, or any external difference observed, except rather a stronger emission of smoke from the interstices of the conical hill at the bottom of the crater. To those who have not visited this romantic and wonderful spot, a slight description of it, as it lately stood, is previously necessary and indispensable to form any conception of it, and to the better understanding the account which follows; for no one living can expect to see it again in the perfection and beauty in which it was on Sunday, the 26th instant.

About 2000 feet from the level of the sea, (calculating from conjecture,) on the south side of the mountain and rather more than two-thirds of its height, opens a circular chasm, somewhat exceeding half a mile in diameter, and

 between

between 4 and 500 feet in depth: exactly in the centre of this capacious bowl rose a conical hill about 260 or 300 feet in height, and about 200 in diameter, richly covered and variegated with shrubs, brushwood, and vines, above half way up, and for the remainder powdered over with virgin sulphur to the top. From the fissures in the cone and interstices of the rocks a thin white smoke was constantly emitted, occasionally tinged with a slight blueish flame. The precipitous sides of this magnificent amphitheatre were fringed with various evergreens and aromatic shrubs, flowers, and many alpine plants. On the north and south sides of the base of the cone were two pieces of water, one perfectly pure and tasteless, the other strongly impregnated with sulphur and alum. This lonely and beautiful spot was rendered more enchanting by the singularly melodious notes of a bird, an inhabitant of these upper solitudes, and altogether unknown to the other parts of the island: hence principally called, or supposed to be, invisible; though it certainly has been seen, and is a species of the merle.

A century had now elapsed since the last convulsion of the mountain, or since any other elements had disturbed the serenity of this wilderness than those which are common to the tropical tempest. It apparently slumbered in primeval solitude and tranquillity, and from the luxuriant vegetation and growth of the forest, which covered its sides from the base nearly to the summit, seemed to discountenance the fact, and falsify the records of the ancient volcano. Such was the majestic, peaceful Souffrier on April the 27th; but we trod on "ignem repositum cineri doloso," and our imaginary safety was soon to be confounded by the sudden danger of devastation. Just as the plantation bells rang twelve at noon on Monday the 27th, an abrupt and dreadful crash from the mountain, with a severe concussion of the earth, and tremulous noise in the air, alarmed all around it. The resurrection of this fiery furnace was proclaimed in a moment by a vast column of thick black ropy smoke, like that of an immense glass-house, bursting forth at once and mounting to the sky; showering down sand, with gritty calcined particles of earth and favilla mixed, on all below. This driven before the wind towards Wallibon and Morne Ronde, darkened the air like a cataract of rain, and covered the ridges, woods, and cane-pieces with light gray-coloured ashes, resembling snow when slightly covered by dust. As the eruption increased, this continual shower expanded, destroying every appearance of vegetation. At night a very considerable degree of ignition

nition was observed on the lips of the crater; but it is not asserted that there was as yet any visible ascension of flame. The same awful scene presented itself on Tuesday; the fall of favilla and calcined pebbles still increasing, and the compact pitchy column from the crater rising perpendicularly to an immense height, with a noise at intervals like the muttering of distant thunder. On Wednesday the 29th all these menacing symptoms of horror and combustion still gathered more thick and terrific for miles around the dismal and half-obscured mountain. The prodigious column shot up with quicker motion, dilating as it rose like a balloon. The sun appeared in total eclipse, and shed a meridian twilight over us, that aggravated the wintry gloom of the scene now completely powdered over with falling particles. It was evident that the crisis was as yet to come—that the burning fluid was struggling for a vent, and labouring to throw off the superincumbent strata and obstructions which suppressed the ignivomous torrent. At night, it was manifest that it had greatly disengaged itself from its burthen, by the appearance of fire flashing now and then, flaking above the mouth of the crater.

On Thursday, the memorable 30th of April, the reflection of the rising sun on this majestic body of curling vapour was sublime beyond imagination—any comparison of the Glaciers, of the Andes, or Cordilleras, with it, can but feebly convey an idea of the fleecy whiteness and brilliancy of this awful column of intermingled and wreathed smoke and clouds: it afterwards assumed a more sulphureous cast, like what we call thunder-clouds, and in the course of the day a ferruginous and sanguine appearance, with much livelier action in the ascent, a more extensive dilatation, as if almost freed from every obstruction:—after noon, the noise was incessant, and resembled the approach of thunder still nearer and nearer, with a vibration that affected the feelings and hearing: as yet there was no convulsive motion, or sensible earthquake Terror and consternation now seized all beholders. The Charaibs, settled at Morne Ronde, at the foot of the Souffrier, abandoned their houses, with their live stock, and every thing they possessed, and fled precipitately towards town. The negroes became confused, forsook their work, looked up to the mountain, and, as it shook, trembled, with the dread of what they could neither understand or describe—the birds fell to the ground, overpowered with showers of favilla, unable to keep themselves on the wing—the cattle were starving for want of food, as not a blade of grass or a leaf was now to be found—the sea was much discoloured, but in no wise uncommonly agi-

 tated;

tated; and it is remarkable, that throughout the whole of this violent disturbance of the earth it continued quite passive, and did not at any time sympathize with the agitation of the land. About four o'clock P. M. the noise became more alarming, and just before sun-set the clouds reflected a bright copper-colour, suffused with fire. Scarcely had the day closed, when the flame burst at length pyramidally from the crater through the mass of smoke; the rolling of the thunder became more awful and deafening: electric flashes quickly succeeded, attended with loud claps: and now, indeed, the hurlyburly began. Those only who have witnessed such a sight can form any idea of the magnificence and variety of the lightning and electric flashes; some forked zigzag playing across the perpendicular column from the crater—others shooting upwards from the mouth like rockets of the most dazzling lustre—others like shells with their trailing fuses flying in different parabolas, with the most vivid scintillations from the dark sanguine column, which now seemed inflexible, and immoveable by the wind. Shortly after 7 P. M. the mighty caldron was seen to simmer, and the ebullition of lava to break out on the N.W. side. This, immediately after boiling over the orifice and flowing a short way, was opposed by the acclivity of a higher point of land, over which it was impelled by the immense tide of liquefied fire that drove it on, forming the figure V in grand illumination. Sometimes, when the ebullition slackened, or was insufficient to urge it over the obstructing hill, it recoiled back, like a refluent billow from the rock, and then again rushed forward impelled by fresh supplies, and scaling every obstacle, carrying rocks and woods together, in its course down the slope of the mountain, until it precipitated itself down some vast ravine, concealed from our sight by the intervening ridges of Morne Ronde. Vast globular bodies of fire were seen projected from the fiery furnace, and, bursting, fell back into it, or over it, on the surrounding bushes, which were instantly set in flames. About four hours from the lava boiling over the crater, it reached the sea, as we could observe from the reflection of the fire and the electric flashes attending it. About half-past one, another stream of lava was seen descending to the eastward towards Rabacca. The thundering noise of the mountain, and the vibration of sound that had been so formidable hitherto, now mingled in the sullen monotonous roar of the rolling lava, became so terrible, that dismay was almost turned into despair. At this time the first earthquake was felt: this was followed by showers of cinders, that fell with the hissing noise of

hail

hail during two hours. At three o'clock, a rolling on the roofs of the houses indicated a fall of stones, which soon thickened, and at length descended in a rain of intermingled fire, that threatened at once the fate of Pompeii or Herculaneum. The crackling and coruscations from the crater at this period exceeded all that had yet passed. The eyes were struck with momentary blindness, and the ears stunned with the glomeration of sounds. People sought shelter in cellars, under rocks, or any where,—for every where was nearly the same; and the miserable negroes flying from their huts, were knocked down or wounded, and many killed in the open air. Several houses were set on fire. The estates situated in the immediate vicinity seemed doomed to destruction. Had the stones that fell been proportionally heavy to their size, not a living creature could have escaped without death: these having undergone a thorough fusion, they were divested of their natural gravity, and fell almost as light as pumex, though in some places as large as a man's head. This dreadful rain of stones and fire lasted upwards of an hour, and was again succeeded by cinders from three till six o'clock in the morning. Earthquake followed earthquake almost momentarily, or rather the whole of this part of the island was in a state of continued oscillation;—not agitated by shocks, vertical or horizontal; but undulated like water shaken in a bowl.

The break of day, if such it could be called, was truly terrific. Darkness was only visible at eight o'clock, and the birth of May dawned like the day of judgement: a chaotic gloom enveloped the mountain, and an impenetrable haze hung over the sea, with black sluggish clouds of a sulphureous cast. The whole island was covered with favilla, cinders, scoria, and broken masses of volcanic matter. It was not until the afternoon, the muttering noise of the mountain sunk gradually into a solemn yet suspicious silence. Such were the particulars of this sublime and tremendous scene, from commencement to catastrophe. To describe the effects is, if possible, a more difficult and truly most distressing task.

THE LATE PHÆNOMENON AT BARBADOES.

PRIVATE LETTER.

"Buttall's Plantation, Parish of St. George, May 2, 1812.

"MY DEAR SIR—I hasten to give you some account of a most awful visitation of Providence which took place yesterday in this neighbourhood, and I believe generally throughout this island.

"Early in the morning, as we imagined from its darkness, my wife requested me to view the sky, which had a very odd appearance. Upon looking at my watch, we were very much surprised to find it was so late as half-past six o'clock A. M. which neither of us could credit, until, on comparing it with hers, they were found to agree.

"In the north-east, in which the sun ought to have been seen, as it had been up three-quarters of an hour, a very large and dense cloud of ferruginous colour, and at no great height above the surface, obscured the firmament, but in such a manner that the trees and shrubs in the garden, and the country to the north and east of our house, presented the same lights and shadows as they usually do when the moon is sometimes bright and sometimes dark in alternation.

"Another cloud, not quite so dense, of a dusky blue colour, and about the same height, hung over the edges of the whole southern horizon, under which the sky appeared of a silvery colour, from which issued a very shining light, by which objects to the northward, when our backs were turned to the light, were seen as distinctly, if not more so, as at noon-day; while objects between us and the light were scarcely perceptible.

"Above these clouds, and in every other part of the sky which was not occupied by the ferruginous clouds, were other clouds of a whitish gray colour, which were carried over the islands with great velocity from the north-east, in which direction the wind blew the whole day with very little variation; although under them there was not a leaf stirring, or breath of air moving.

"A solemn and unusual stillness pervaded every place, now and then interrupted by the sound of negroes at work with their hoes, which the surrounding silence seemed to augment.

"Forcibly struck with all these appearances, I could not help expressing to Mrs. D. my apprehensions that this island was about to be visited with some dreadful commotion; and as our house was on the acclivity of a hill, about sixty feet above the level of the works, at which our friends Mr. and Mrs. H. resided, I thought it advisable we should join them; which we did without delay.

"By the time we reached the works it was seven o'clock; and as the darkness began to increase considerably, all the negroes were recalled from the fields, and ordered to their houses, where most of them went to bed with much indifference, considering the darkness only as an early night.

"At half-past seven o'clock it was so dark that candles were

were brought in. At eight o'clock it was pitch dark in the open air; or, in other words, so dark that we could not perceive our hands when held up before our faces at two feet distance. No night at home in winter, when neither the moon nor a star is to be seen, was ever more sombre. This darkness continued of the same intenseness until 25 minutes past twelve o'clock—that is, for the space of four hours and 25 minutes, at which time we perceived very indistinctly the outlines of large and near objects. At half-past twelve o'clock we distinguished them more correctly; from which period the light increased until between three and four o'clock P. M. but was very obscure.

"From the time at which I got up in the morning until we went to bed at home in the evening at eight o'clock there was a constant fall from the clouds of a substance in extremely fine flakes, which when first gathered from our clothes had the appearance of the dust of wood-ashes; but which, when suffered to accumulate, assumed the resemblance of powdered rotten-stone, and possessed the same quality of cleaning brass.

"In order to ascertain the quantity which had fallen, Mr. H. last night took up that which lay upon a foot square, when it measured three pints, somewhat pressed into the measure, and weighed one pound and three-quarters.

"This morning another square foot, where the surface was hard and level, gave, in five eighths and one half of an inch in depth, three pints loosely filled up in measure, and one pound seven ounces and a half in weight.

"Against the bottom of windows, doors, and walls, it was considerably deeper. But assuming the product of my experiment as the medium quantity which fell on a foot square throughout the island, and estimating from our best maps the quantity of land in the island at 106,470 acres*, the total quantity of this extraneous substance which is now on its surface, independent of that which is upon the trees, could not be less than 1,739,187,750 gallons, wine measure†, or 6,811,817,512 pounds avoirdupois‡.

	Acres.		Acres.		Acres.
* Christchurch parish,	14,310	St. John . . .	8,600	St. Andrew . .	8,780
St. Philip	15 040	St. James . . .	7,800	St. Peter . . .	8,330
St. Michael	9,580	St. Thomas .	8,500	St. Lucy . . .	8,725
St. George	10,795	St. Joseph . .	6,010		

† 106,470 acres in the island, multiplied by 43,560 square feet in one acre, is equal to 4,637,833,200 square feet, multiplied by 3 pints per foot, is equal to 13,913,499,600 pints, divided by 8 pints in a gallon, is equal to 1,739,187,450 gallons, wine measure.

‡ 4 637,833,200 square feet in the island, multiplied by 1 lb. per inch, 7½ c. feet, is equal to 6,811,817,512 lbs. avoirdupois.

"The

" The fall of this substance was least in the morning and evening, and greatest between nine and twelve o'clock. During this last period, when any of us went out into the air, we perceived a smell similar to that which arises from water thrown upon hot embers—but with no increase of heat, to the best of my perception: on the contrary, according to my own sensations, it was rather colder during the continuance of this phænomenon than in common.

" I had unluckily left my thermometer behind me, and did not get it until one o'clock P. M. at which time I hung it in a gallery facing the south, and open to the air, when it settled at $77\frac{1}{2}$, and continued at that height until five o'clock in the afternoon, after which I forgot to examine it.

" All those who ventured out with lanterns during the darkness reported they had heard strange noises and cries in the air; but as they also added, that these noises and cries had followed them, and that objects had been seen or felt flitting past them, it was no difficult matter to convince most of them that these sounds arose from the birds and bats, which the lights they carried with them had attracted. Many animals, which had been loose at the commencement of the darkness, found their way to the lights in the houses, and remained at the house during the continuance of the darkness.

" Some land birds flew into the rooms where the windows were open, and some few sea birds were known from their cries to be hovering about the buildings.

" After this dismal scene had continued some time, my apprehensions were considerably lessened, by bringing to my remembrance the account of the younger Pliny of the eruption of Vesuvius; and the fact mentioned to me by yourself, of a substance similar in some degree to what has fallen here, having been gathered off the sails of a ship, at a great distance from any known land; from whence I began to hope, that as no cinders had made their appearance, the phænomenon, which was alarming us, might be the effect of some distant volcanic eruption, and not the forerunner of any more dreadful visitation than that which was existing at the moment.

" This idea has been strengthened by the following circumstances:

" About one o'clock in the morning Mr. H—— and several others on the plantation heard a very heavy and quick firing, neither as minute guns from a ship in distress, not in continuance as from ships engaged, but in peals at intervals, from the southward. The same firing was heard so distinctly

stinctly in town and its vicinity, but in a westerly or north-westerly direction, that it is said our Governor, who is also commander of the forces in the leeward island, repaired about two o'clock to the garrison, which was kept under arms all the night, from a surmise that Admiral Sir Francis Laforey, who had sailed to the northward the evening before at sun-set with the Dragon and a tender only, had fallen in with the enemy's fleet of four sail of the line and four frigates, which, by accounts from Madeira, are reported to have passed that island.

"As nothing has transpired this forenoon to countenance either the supposition of an engagement, or of a ship having been lost last night off our coast, it is very possible, and by no means improbable, that the noise in question may have proceeded from a volcanic eruption, which, without any earthquake having been perceptible in this island, may have produced those phænomena which I have attempted to delineate.

"During last night the weather has been rather calm than otherwise; and this morning the appearance of the country resembles the land in the neighbourhood of the river Nith, near its entrance into the Solway frith, when it has been overflown by a high tide. Many shrubs and low trees with spreading branches have been split by the weight of the substance which has fallen on them, and the sugar-canes are bent down to the ground.

"On removing the substance from the surface, the grass continues as green at least as it was before, without any appearance of being scorched.

"The thermometer this day, when placed in a room open to the air, but not exposed to the direct or reflected rays of the sun, has varied from 70 to 80, which is three degrees higher than it was yesterday at the same hour of the day.

"Being now relieved from the darkness and its concomitant fears, we are beginning to distress ourselves about the effect which the fallen substance is likely to produce on the fertility of the land: to ascertain which with as little delay as possible, my father in-law and I have this morning planted various seeds, useful and ornamental, in this substance singly, and also in it when combined with different proportions of the soil; trusting that the same good Providence, which has carried us in safety through the danger of yesterday, may be pleased to render this visitation a blessing and not a curse to the island.

"Lest you may wish to ascertain the component parts of this substance, I will send you by the first good opportunity the

the portion of it I gathered from the surface of a foot square, as I have already noticed, and in the interim I inclose you a few grains.

"Yours, &c."

LECTURES.

Mr. Wilson and Mr. Charles Bell will in future deliver the Lectures on Anatomy, Physiology, Pathology, and Surgery, conjointly, at the Theatre of Anatomy, Great Windmill-Street. The Lectures are so arranged that the subjects delivered during the Autumnal Course, by the one, will, in the Spring, be given by the other. Each will, therefore, during the Season give one complete Course of Lectures.

The Museum of Windmill-street, now consisting of the united Collections of Mr. Wilson and Mr. Bell, may be seen by Gentlemen of the Profession during the vacation months, by applying to Mr. Bell, 34, Soho-suare.

LIST OF PATENTS FOR NEW INVENTIONS.

To Leger Didot, late of Paris, but now of Two Waters in the county of Hertford, gentleman, the original inventor of certain machines for making wove and laid paper, who communicated such inventions to John Gamble and Henry Fourdrinier, and who is the foreigner referred to in the several patents obtained by them in April 1801, and July 1806, for certain further improvements upon the said machines for making both wove and laid paper.—26th May, 1812.

To William Hardcastle, son of George Hardcastle, late of Derby, worsted spinner, deceased, (in consequence of a communication made to him by his said father a sort time previous to his death,) for an improvement on cranes to prevent accidents from the rapid descent of heavy bodies. —26th May.

To John Scambler, of Birmingham, needle manufacturer, for improvement in the manufacturing of needles.—2d June.

To Leger Didot, of Paddington, in the county of Middlesex, gent. for certain improvements in the method or means of illuminating apartments and other places; namely, in candlesticks to be used with candles manufactured in the usual manner, or otherwise; and in candles to be burned with peculiar advantages in candlesticks so improved; and also in snuffers to be used with such candlesticks.—2d June.

To Henry Thomas Hardacre, of Gloucester Place, New Road,

Road, Middlesex, esq. for his composition to prevent to a very extensive degree the effects of friction.—6th June.

To James Lee, of Frizo Water House, Enfield Wash, Middlesex, gentleman, for certain new methods of preparing hemp and flax for their various uses, and by which also other vegetable substances may be rendered applicable to many of the purposes for which hemp and flax are used. —9th June.

To James Needham, of Islington, corn dealer, for certain additions to and improvements on his patent portable apparatus for brewing beer from malt and hops, whereby the same can be applied to other useful purposes. —9th June.

To Benjamin Black, of George Street, Grosvenor Square, Middlesex, carriage-lamp maker, for various improvements in the construction of coach, chariot, and other carriage lamps.—25th June.

METEOROLOGY.

Meteorological Observations made at Clapton in Hackney, from June 21 to July 20, 1812.

June 21.—W.S.W.—S.W. Hard showers with fair intervals, in which confused *cirrus, cumulostratus,* &c. were seen. Fine evening.

June 22.—S.W. Fair day; *cumulostratus* prevailed; in the evening features of flimsy ill-formed *cirrus,* and *cirrocumulus* by night.

June 23.—W.S.W. Fair day, with *cumuli* and *cumulostrati* petroid and mountainous; also some features of *cirrus,* &c.

June 24.—W.S.W.—N.W. Confused *cirrus* spread about aloft; *cumuli* lower, *cumulostratus,* and a slight shower about noon; many *nimbi* seen above, but no other fell in rain hereabouts.

June 25.—Much cloud of ragged *cumulostratus,* reddened by the setting sun; *cumulostratus,* &c. and very cool; some rain had fallen during the night, but the day was fair, with petroid *cumuli,* &c.

June 26.—N.E. Rainy and windy morning; various currents of air blew together about noon, followed by thunder and lightning; the rain continued nearly all day; and the night was cloudy, cold, and damp, with N. wind.

June 27.—N.E. *Cumulus, cumulostratus,* and some scattered *cirrus,* with heavy clouds covering the sky by night.

June 28.—N.E. Showers; petroid *cumulostrati,* scud, &c.; fair night.

June

June 29.—N.W. Clear morning; *cumuli*, and some ill-defined *cirri* in the day; rainy appearances at night.

June 30.—W. Sky for the most part clouded, the sun only breaking out at times. Very cool and winterlike for the time of year.

July 1.—W. Cloudy and rainy by times, and very cold.

July 2.—W. Cloudy, with some rain in the morning; the day became fine with rainy features of *cumulus*, *cirrus*, &c.

July 3.—W.N.W. Cloudy morning; fair day, with slight showers, and fine evening; the clouds were petroid *cumuli* and *cumulostrati*, with some *cirrus*, *cirrocumulus*, and *cirrostratus*; in the evening the *cirrocumulus* was lofty, flimsy, and semitransparent.

July 4.—Clear early, then petroid *cumuli*, with fibrous and brushlike *cirri* above; *cumulostratus* also formed, and indeterminate features of a sort of *cirrocumulus* very large and irregular; clear evening, with some clouds; wind S.

July 5.—Clouds of mixed and indeterminate modification, followed by scattered *cirrus*, *cumulus*, &c.; wind S. and S.W.

July 6.—N.W. Flimsy masses of cloud early; fair day, with fibrous *cirri* stretched along above *cumuli*.

July 7.—W. Hot day; therm. 79°. Features of *cirrus*, *cumulus*, and *cumulostratus*; the under surface of which had often a wavy appearance. In the evening the lofty and spread masses of cloud showed a tendency to *cirrocumulus*, with large *nubeculæ*, such as I have frequently noticed on warm evenings, but they were not very well defined. Some *cirrocumuli* were flocky and of smaller *nubeculæ*.

July 8.—S.—S.E. Clear morning; the first clouds were flimsy; *cirri* passing to irregular *cirrocumuli*, afterwards *cumuli* and *cumulostrati*; very clear evening again, and fine yellow sunset. Thermometer in shade when highest 73°. By night an obscuration of the stars' light occurred.

July 9.—S.E.—E. Cloudy morning, clear day, with *cumuli*; fine clear evening, and yellow colour at sunset. Therm. 73°; like yesterday.

July 10.—Clear hot morning, with east wind; some long and light masses of cloud showed a tendency to *cirrocumulus*, afterward *cumuli* and *cumulostrati*. The wind became north-east in the afternoon, got high, and blew over clouds which obscured the sky. I observed that the *cumuli* in the fine part of the day were hazy and ill-defined.

July 11.—Clear early, then clouded, and somewhat hazy; it cleared again, and various small *cumuli* prevailed; wind still N.E.

July

July 12.—N. *Cumulostratus* in the morning, with nimbification, afterwards fair, *cumuli* and *cumulostrati*, with a disposition above to *cirrocumulus*. By sunset the under surface of a mass of a kind of *cumulostratus* refracted a rich crimson light, from which hung golden dependent fringes; the sky beneath of a fine yellow; clear and cool night.

July 13.—N.E. Clear early, then *cumuli* formed; small and fleecy specks by degrees became larger; neighbouring ones were lost, and the *cumuli* became large, but ill-defined; some higher masses of *cirrostratus* and *cirrocumulus* ill-defined.

July 14.—In the morning I saw *cumulostrati* below *cirrocumulus*; wind S. E. In the afternoon a *nimbus* came up from W.N.W., after which the wind blew from that quarter; clear night.

July 15.—*Cumuli*, with fibrous *cirri* higher up; they ramified in various directions; those which appeared the lowest were more comoid, and some extended their fibres in every direction; air dry, and wind W.N.W. Towards evening the *cirri* became more confused, passed to *cirrocumulus*, and in some places to *cirrostratus*, through which the moon appeared obscurely, and predicted rain; but the night became tolerably fair; wind westerly.

July 16.—Clouds followed by small rain, which held up after noon, and the evening was fair; wind S.

July 17.—Wind S. and S. E.; warm day; atmosphere rather misty, with clouds in different stations, and ill-defined *cumulostratus*; fleecy *cumulus*; and in the upper regions a tendency to *cirrocumulus*; upper current of air by passing clouds from the north at $6\frac{1}{2}$ p.m.

July 18.—Fair hot day; *cirrus* continued forming aloft, of nimbiform appearance; in the evening it approached to the nature of *cirrostratus*, and exhibited itself in fine streaks, like the grain of polished wood, and in rows like the mackerel back feature well known by that name. *Cumuli* occasionally thickening into almost *cumulostratus* also prevailed; some tendency also appeared to *cirrocumulus*. Wind westerly.

July 19.—Cloudy morning; *cumulostratus* beneath a veil of cloud. Showers in the evening. W.

July 20.—Showers; some of them accompanied by high wind and thunder; in the clear intervals confused and irregular *cirrus* and *cirrocumulus*, with *cumuli*, scud, &c. Wind westerly.

Clapton, July 22, 1812. THOMAS FORSTER.

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For *July* 1812.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
June 27	54	62°	52°	29·80	36	Fair
28	50	58	49	·85	40	Fair
29	54	70	55	30·07	46	Fair
30	60	70	56	29·89	24	Showery
July 1	58	62	57	·62	0	Rain
2	59	60	55	·70	46	Showery
3	54	59	49	·89	47	Fair
4	52	60	55	30·02	40	Showery
5	56	62	53	·01	36	Showery
6	61	66	56	·20	60	Fair
7	58	73	59	·30	73	Fair
8	60	72	55	·35	68	Fair
9	57	73	60	·32	81	Fair
10	64	70	57	·33	66	Fair
11	60	68	58	·36	64	Fair
12	59	65	52	·14	45	Fair
13	54	64	58	·15	70	Fair
14	60	65	50	·17	66	Fair
15	55	68	58	·12	88	Fair
16	58	66	60	29·92	0	Rain
17	62	68	60	30·00	56	Cloudy
18	66	76	63	·03	62	Fair
19	64	66	62	29·80	23	Rain
20	64	66	60	·70	29	Stormy
21	60	69	56	·90	60	Fair
22	60	67	52	·98	30	Stormy
23	55	68	56	·92	57	Fair
24	56	65	62	·75	0	Rain
25	65	69	60	·74	39	Fair
26	64	70	61	·75	70	Fair

N.B. The Barometer's height is taken at one o'clock.

XVI. *Description of a new Thrashing Machine. By* RICHARD TAWNEY, *Esq.*

To Mr. Tilloch.

SIR, IT having been long thought in this part of the kingdom a desideratum to unite in a thrashing machine with a small power the qualities of clean and good work; the following description, accompanied by a drawing of one which a short time since I had made for my own use, and which most effectually answers the purpose, will, perhaps, be not unworthy a place in the Philosophical Magazine.

I was induced to the attempt by observing that in every machine in this neighbourhood, both fixed and portable, a very large portion of the power was lost by friction, seldom less than half, and wished to try what could be effected by a machine constructed with a view to reduce the friction as much as possible.

The drawing presents an elevation and section of a machine to be worked by two men, with each a winch upon the axis of a wheel (which, as they could not be well shown in a geometrical view, are purposely omitted) turning by a small rope a pulley fixed upon the axis of the drum.

It will be seen that the wheel is hung upon friction rollers, and it was my intention to have put rollers to the drum axis as well; but as these were found to enhance the price, and thereby defeat the general utility of the plan, the latter were omitted, and the friction kept down by making the brasses and axis as perfect and as small as they well could be with safety.

It should be mentioned that the drum, rollers, and plates, are upon the principle of the machines made by a Mr. Wilson, of Leicester, which for quantity and goodness of work are the best of a great number that I have seen. In these machines, as will be observed in the section, the feed rollers are placed horizontally, and they are fed by laying the corn upon a board inclining to them from the top. One roller is turned by a small strap from the drum axis; and as the two are made to touch by a thin well turned iron wheel or flanch at each end, the other roller is moved by friction.—Lest this should be thought strange in a machine purposely made to avoid friction, it must be remembered that, as the wheels are small, the points of contact are trifling, and that there are no other means of obtaining an equable motion so good.

The axis of the second roller is hung at each end upon a pivot, so that when the feed is too great the roller will open, and be brought back again by a weight suspended at the end of a small lever.

The drum is made open, and the beaters fixed upon a ring at each end.

My machine is greatly improved by giving the drum (as well as the large wheel) a fly motion. The rings are made thin and light of wood, and the beaters of bar iron. The more simple way perhaps would be to make the rings of cast iron, and throw the weight to the rim.

The leading feature of Mr. Wilson's machine is the situation of the parallel ribbed plates against which the corn is beaten or rubbed. Compared with other machines, they may be said to be divided: one part is fixed upon the levers which carry the feed rollers, the distance of which is regulated by a screw through the top of the frame;—the other is fixed in the frame which carries the iron grating, and with that the distance is regulated by a screw at each corner.

It will be seen that the straw after passing the first plate may take a different direction before it enters upon the second plate and grating, and to this circumstance I think is to be attributed the machine's superior work. By placing the feed rollers so far from the ribbed plate, and beaters, the straw is not at all broken, but will admit of being tied up in boltings, as well as if thrashed by the flail.

The great wheel of this machine is six feet six inches diameter—to avoid resistance from the air, it is but two inches in thickness, and the spokes are made elliptical. To give it a fly motion, fifty-six pounds of cast iron are added to the ends of the spokes, as described in the elevation. This weight is sufficient to lead the men on, so that the difficulty is not to keep them up to a certain number of revolutions per minute, but to dissuade them from doing too much.

The pulley hitherto worked upon the drum has been either one-eighth or one-tenth the diameter of the wheel; and as in working the former is lighter, I find the velocity of the hand increased in proportion. The minimum velocity of the hand is forty revolutions per minute, and this carries the drum through a greater space than that of any ordinary machine of two or four horse power.

The winches at first were made to describe a circle of thirty-six, but were subsequently shortened to one of thirty inches diameter.

The

The rope is of the best tarred kind one inch and a half in circumference, and spliced together.

The carriages of the wheel are each fixed to the frame by two screw pins, and the variation of the rope is remedied by slackening of the nuts.

What this machine will thrash when corn yields well, I am not prepared to say.

It has been tried with the worst of every sort of grain, and thrashed it perfectly clean.

With the labour of two men, and that about the same as when using the flail, it thrashed of the last season from thirty to thirty-six bushels (eight and half gallon measure) of wheat per day, which will warrant an expectation of considerably more in a good yielding year.

In the late wet spring I found it exceedingly useful to employ my men on those days when nothing out of doors could be done; and although with a winch it may be not quite applicable to the case of every large farmer, still I am persuaded a considerable part of the farming interest will find it of real advantage as an auxiliary, if not a principal machine, and to the small occupier I can recommend it as the most effective hand machine yet in use.

I am, sir,

Your most obedient servant,

Dunchurch Lodge, May 2, 1812. RICHARD TAWNEY.

XVII. *An Account of some Experiments on the Combinations of different Metals and Chlorine, &c. By* JOHN DAVY, *Esq. Communicated by Sir* HUMPHRY DAVY, *Knt. LL.D. Sec. R.S.*

[Continued from p. 12.]

3. *On the Combinations of Iron and Chlorine.*

As there are two oxides of iron, so there are also two distinct combinations of this metal and chlorine. One may be directly formed by the combustion of iron wire in chlorine gas; it is that volatile compound described by Sir Humphry Davy in his last Bakerian Lecture, which condenses after sublimation in the form of small brilliant iridescent plates. The other, I find, may be procured by heating to redness, in a glass tube with a very small orifice, the residue which is obtained by evaporating to dryness the green muriat of iron; it is a fixed substance requiring a red heat for its fusion; it is of a grayish but variegated colour, of of a metallic splendour, and of a lamellar texture. As it absorbs chlorine when heated in this gas, and becomes entirely converted into the volatile compound, and as the volatile compound may likewise be obtained by heating, in a glass tube nearly closed, the residue from the evaporation of the red muriat, it is evident that the fixed compound contains less chlorine than the volatile, and that the former, consequently, may be called ferrane, and the latter ferranea.

Ferrane dissolves in water, and forms the green muriat of iron; but the solution of the whole substance is not complete. There is always left a small and variable quantity of black oxide, which may be considered, on account of its variability, in a state of mechanical mixture, rather than of chemical union with the ferrane.

Ferranea is entirely soluble in water. The solution is identical with the red muriat of iron.

The analysis of both these compounds is easily effected by means of nitrat of silver.

Fifty grains of ferrane were put into water: the insoluble residue separated from the solution by decantation; washed, dried, and heated to redness for a minute, previously moistened with oil, weighed 3 grains, and was in the state of the black oxide, being attracted by the magnet. The solution entire, precipitated by nitrat of silver, afforded 102·5 grains of dried horn silver, which indicating 25·1125 grains of chlorine, the proportion of iron, omitting the 3 grains of oxide, appears to be 21·8875. And hence 100 of ferrane seem to consist of

53·43	chlorine
46·57	iron
100·00	

Ferranea is not easily obtained in considerable quantities, I have been obliged in consequence to operate upon small portions. The subject of analysis was procured by sublimation from the residue by evaporation of the red muriat. 20 grains of this, in brilliant scales, were weighed in water. The solution, precipitated by nitrat of silver, yielded 53 grains of dried horn silver. Hence 100 of ferranea appear to consist of

64·9	chlorine
35 1	iron
100·0	

4. *On the Combinations of Chlorine with Manganese, Lead, Zinc, Arsenic, Antimony, and Bismuth.*

I have attempted, by several methods, to obtain more than

than one combination of these different metals and chlorine, but without success.

I have procured a compound of manganese and chlorine, by evaporating to dryness the white muriat of this metal, and heating to redness the residue in a glass tube, having only a very small orifice. Muriatic acid vapour was produced, and a fixed compound remained, which required a red heat for its fusion, and was not altered by the strongest heat that could be given to it in the glass tube; but was rapidly decomposed when heated in an open vessel, muriatic acid fumes being evolved, and oxide of manganese formed, which was black or red, according to the intensity of the heat applied. The compound of manganese and chlorine is a very beautiful substance, it is of great brilliancy, generally of a pure delicate light pink colour, and of a lamellar texture consisting of broad thin plates.

There is not much difficulty in obtaining this compound pure. Iron, with which manganese is commonly contaminated, may be separated by two or three repetitions of the solution of the compound in water, the evaporation to dryness of the clear filtered muriat, and fusion of the residue procured by evaporation. Indeed, I think this a good general method for purifying manganese from iron. One of the combinations of the latter metal and chlorine being volatile, heat must separate it from the compound of manganese. And I have thus obtained it so free from iron, that triple prussiat of potash added to its solution in water gave merely a white precipitate without the slightest tint of blue.

This compound deliquesces when exposed to the atmosphere, and is converted into the white muriat. Like ferrane, it affords a trifling residue when heated with water. The residue is oxide of manganese, white at first, but soon becoming red, and even black; it varies in quantity, according to the exclusion of air in the formation of the combination.

Fifty grains of the compound dissolved in water, with the exception of one grain; this residue was separated by decantation of the fluid, washed, dried, and heated to redness, it was in the state of black oxide. The colourless solution was precipitated by nitrat of silver. The horn silver formed, when dried, was equal to 108 grains. Hence, omitting the one grain of mixed oxide, 100 of this compound appear to consist of

54 chlorine
46 manganese
100

The

The horn lead that I have analysed was made by the decomposition of the nitrat of lead by muriatic acid, and it was well washed, dried, and fused in a glass tube with a small orifice. The strongest red heat that I could apply to it, under these circumstances, did not occasion its sublimation.

Fifty grains of it that had been fused were dissolved in water. This solution, heated with nitrat of silver, afforded 52·65 grains of dry horn silver. Hence 100 of horn lead appear to be composed of

25·78 chlorine
74·22 lead

100·00

As this compound, when decomposed by an alkali, affords the protoxide of lead, it may be called plumbane.

The butter of zinc I have examined was obtained by evaporating to dryness the muriat of this metal, and by heating to redness the residue in a glass tube. This compound is not volatile at a strong red heat in a close vessel, it fuses before it acquires a dull red heat, and on cooling it goes through several degrees of consistency, being viscid before it becomes solid.

This compound, when heated with water, affords a small residue of oxide of zinc, which, as in the preceding instances, may be considered as in the state of mechanical mixture.

In consequence of its powerful attraction for water, it is a very deliquescent substance; on this account it is necessary to weigh it in water to avoid error. 49·5 grains of it thus weighed, dissolved entirely in water, with the exception of one grain of oxide of zinc, which was separated by decantation and dried and ignited, and its quantity ascertained to be as stated. The solution precipitated by nitrat of silver afforded 99 grains of dried horn silver. Hence, excluding the one grain of oxide, 100 of butter of zinc seem to consist of

50 chlorine
50 zinc

100

This compound may be called zincane.

A compound of chlorine and arsenic has been long known, bearing the name of the fuming liquor of arsenic. It may be formed in several ways; by the combustion of arsenic in chlorine gas, by heating in a retort a mixture of arsenic

arsenic and corrosive sublimate, or of arsenic and calomel, and by the distillation of muriat of arsenic with concentrated sulphuric acid. The old method by means of corrosive sublimate appears best adapted for procuring it in a pure state. About six parts of corrosive sublimate to one of arsenic are, I find, proper proportions. The mixture of the two substances should be intimate, and the heat applied to the retort for the distillation of the fuming liquor, gentle. When the liquor was not colourless at first, I have purified it by a second distillation.

The fuming liquor of arsenic, it is well known, is decomposed by water. The precipitate produced appears to be merely white oxide of arsenic; for, independent of other circumstances, it does not afford the fuming liquor when heated with strong sulphuric acid.

The fuming liquor, when gently heated, dissolves phosphorus, but it retains on cooling only a very small portion of this substance. The warm solution is not luminous in the dark.

The fuming liquor also, when warm, readily dissolves sulphur; indeed sulphur fused in the liquor seems capable of combining or of mixing with it in all proportions; but on cooling the greatest part of the sulphur is deposited, and assumes a fine crystalline appearance; the form of the crystals was apparently the octahedron. This deposition seems to be merely sulphur with a little of the fuming liquor between the interstices of the crystals; for the crystals bear washing, and become tasteless superficially, but remain still acid internally, where the water has not penetrated.

It likewise dissolves resin. That which was called rosin was the subject of experiment. The solution was of a blueish green colour; but when gently heated it became brown, and remained so on cooling. The portion of resin the fuming liquor is capable of taking up is very considerable; when the resin was added in excess, a viscid mixture was formed. The resinous solution was decomposed by water, and the resin was separated apparently unaltered mixed with white arsenic.

The fuming liquor is capable of combining with oil of turpentine and with olive oil. When the mixture was made with either of these oils, there was a considerable elevation of temperature, and a homogeneous colourless fluid was in each instance obtained.

In these and some other properties, the fuming liquor of arsenic is analogous to the fuming compounds of chlorine

 and

and sulphur, and chlorine and phosphorus; these, too, having the power of dissolving sulphur, and phosphorus, and resin, and of entering into union with the fixed and volatile oils.

It is difficult to ascertain the proportion of the constituent parts of this compound by the ordinary modes of analysis. I have chosen therefore a synthetical method in preference; and from repeated experiments I find that two grains of arsenic require for complete conversion into the fuming liquor, four cubic inches exactly of chlorine gas.

The experiments were thus conducted: the arsenic in one piece was put into a small glass retort having a stop-cock, the retort was exhausted, and a known volume of chlorine gas was admitted from a graduated receiver by means of other stop-cocks, and the absorption of chlorine, after the entire conversion of the metal into the fuming liquor, was considered as the proportion condensed by the arsenic.

Now, since 100 cubic inches of chlorine gas weigh just 76·5 grains, two grains of arsenic combine with 3·06 grains of chlorine, the weight of four cubic inches of the gas. Hence 100 of the fuming liquor appear to consist of

60·48 chlorine
39·52 arsenic

100·00

As the fuming liquor gives the white oxide when decomposed by water, arsenicane may be substituted for its old name.

The butter of antimony is a well known substance. That which I have examined was obtained by heating together corrosive sublimate and antimony, or antimony and calomel; and was always purified by a second distillation at a low temperature. The best proportion of corrosive sublimate and the metal for making the compound, I have found to be 2½ parts of the former to one part of the latter.

The butter of antimony, like arsenicane, is capable, when rendered fluid by heat, of dissolving resin and sulphur, and of combining with the fixed and volatile oils. It affects the oil of turpentine very like the liquor of Libavius; the action is considerable, much heat is produced, and the oil is rendered brown.

When the butter of antimony is decomposed by a sufficiently large quantity of the hydrosulphuret of potash, that compound is formed which is commonly called the golden sulphur of antimony, and which when decomposed by heat

I have

I have found to afford merely water and sulphuret of antimony *.

To ascertain the proportion of antimony in the butter of antimony 60·5 grains of this substance colourless and crystallized, weighed in water, were heated in a solution of hydrosulphuret of potash. The whole of the antimony was dissolved, and the hydrosulphuret of potash being in excess, there was no precipitation on cooling. The solution was decomposed by muriatic acid, and the golden sulphur thus thrown down was collected on a filter, well washed and dried; heated slowly to redness in a glass tube, steam in plenty was disengaged with very slight traces of sulphur, and sulphuret of antimony remained, which fused into one mass weighed 45 grains. According to the experiments of Proust, which I have repeated with the same result, sulphuret of antimony contains 74·1 per cent. of metal. Hence 45 grains of sulphuret or the 60·5 of butter of antimony, from which the sulphuret was procured, must contain 33·35 of metal; and considering the remainder 27·15 of the 60·5 as the proportion of chlorine, 100 of the butter of antimony seem to consist of

39·58	chlorine
60·42	antimony
100·00	

This compound, as it yields when decomposed by water the submuriated protoxide, may be called antimoniane or stibiane.

A compound of bismuth and chlorine has been long known bearing the name of the butter of bismuth. It is obtained both when bismuth is heated with corrosive sublimate and calomel. Two parts of corrosive sublimate to one part of metal, I have found good proportions for its preparation. There is some difficulty in procuring it pure and entirely free from the mercury revived; this is most readily effected by keeping the butter of bismuth in fusion, at a temperature just below that at which mercury boils; the mercury slowly subsides and collects in the bottom of the vessel,

* These results appear to me to demonstrate the truth of M. Proust's opinion, that the golden sulphur is a hydrosulphuretted oxide of antimony. From my experiments, the only difference of composition between kermes mineral and the preceding compound seems to consist in the former containing a smaller proportion of sulphuretted hydrogene than the latter; for I have obtained by the decomposition of kermes mineral, by heat, a compound of sulphuret of antimony and protoxide, and I have converted kermes into the golden sulphur by means of water impregnated with sulphuretted hydrogene.

vessel, and this operation continued for an hour or two affords a pure or nearly pure butter of bismuth. Thus prepared, it is of a grayish white colour, opake, uncrystallized, and of a granular texture. In a glass tube, with a very small orifice, it bears a red heat without subliming.

As a hydrosulphuret of bismuth is produced when the butter of bismuth is heated with the hydrosulphuret of potash, and as this hydrosulphuret, like that of antimony, affords, when decomposed by heat, a sulphuret and water, I have applied the same mode of analysis to this compound as to the last.

Fifty-five grains of butter of bismuth were decomposed in a warm solution of hydrosulphuret of potash. The dark brown hydrosulphuret of bismuth thus formed, and not dissolved, was collected on a filter; the hydrosulphuretted solution was decomposed by muriatic acid, the slight precipitate of hydrosulphuret produced was added to the first portion, and the whole was well washed, dried, and heated to redness in a glass tube; the sulphuret of bismuth thus obtained, fused into one mass, weighed 44·7 grains. I had previously ascertained the proportion of metal in this sulphuret, and found it to be 81·8 per cent. 44·7 grains of sulphuret, or 55 grains of the butter, must therefore contain 36·5 grains of bismuth; and hence, 100 of bismuth appear to consist of

33·6 chlorine
66·4 bismuth

100·0

The butter of bismuth may be called bismuthane.

Among the preceding combinations of the metals and chlorine, there is a surprising difference in respect to volatility and fusibility. Iron and manganese, two difficultly fusible metals, form with chlorine readily fusible compounds, and a combination of the former metal and chlorine is even volatile; the compounds of tin and chlorine, and of chlorine and antimony, are very volatile substances, though the metals themselves are fixed at very high temperatures; on the contrary, the combinations of chlorine with bismuth, zinc, and lead, do not exceed in fusibility, indeed are not quite so fusible as the metals themselves. I can offer no explanation of these phænomena.

Another singularity attending the liquid fuming compounds of chlorine, such as the liquor of Libavius, the fuming liquor of arsenic, and the oxymuriats of sulphur and phosphorus, is, that they do not become solid at low temperatures.

temperatures. I have reduced, by means of a mixture of snow and muriat of lime, the temperature of all these substances 20 degrees below the zero of Fahrenheit's thermometer, but without affecting their liquidity.

The influence of atmospheric air on the compounds of the metals and chlorine at high temperatures is curious, and worthy of particular attention. The combinations of chlorine with lead, zinc, copper, and bismuth, appear to be volatile in open vessels, and fixed in closed ones. How moist air operates in these instances, it is difficult to say. In other cases, where it evidently acts chemically, the changes explain themselves; thus, when the compounds of iron and chlorine and of manganese and chlorine are heated in the open air, hygrometrical water of the atmosphere seems to be decomposed, as muriatic acid fumes are produced, and oxides of the metals formed. Probably the volatility of the other compounds is connected with similar circumstances. This action of moist air has hitherto been much neglected; it is certainly worthy of being more fully inquired into, both in a theoretical and practical point of view. Its importance in practice is exemplified in the reduction of horn silver, and in the formation of several of the compounds of chlorine and the metals; if moist air be admitted in these operations, the silver will be lost, and the compounds not formed.

Guided by analogy, I have been led to try whether the muriat of magnesia, which is readily decomposed by heat in the open air, would not, when the air was excluded, by introducing it into a glass tube with a very small orifice, afford a permanent compound. The result was agreeable to my expectations; I obtained, by strongly heating the muriat for a quarter of an hour, a substance like enamel in appearance, being semi-fused, and which appeared to be a mixture of magnesia and the true compound of magnesium and chlorine; for heated with water magnesia was separated, and a muriat of magnesia formed.

[To be continued.]

XVIII. *Extract from a Report made to the Royal Institution on* MASSEY'S *Sounding Machine, March* 4, 1811.

To Mr. Tilloch.

SIR, IN March 1811, it fell to my lot as chairman of the Committee of Mathematics and mechanical Inventions at the

the Royal Institution, to prepare and make a Report on the new Sounding Machine invented by Ed Massey. My object in making that Report was to put upon record the origin and progress of an invention which has been universally adopted by the British navy; but as I find it does not enter into the views of the managers of the Royal Institution to give it that publicity which I proposed, I have therefore sent you, sir, an extract of the Report, in hopes that this valuable invention may, through the extensive circulation of your Magazine, meet with that publicity which it so highly deserves;

and am, sir, your obliged servant,

Welbeck street, Aug. 3, 1812. ROBERT CLIFFORD.

Extract from the Report, &c. &c.

THE mechanical invention, which the Committee of Mathematics, Mechanics, and mechanical Inventions have selected, and to which they wish to give every possible degree of publicity, is one which must necessarily contribute to the preservation of the lives of our fellow subjects, and at the same time, to the rendering that branch of British power, the navy, still more secure. It is a machine for taking correct soundings at sea; but previous to the description of the machine, it will be proper to examine the question of soundings in general.

Soundings are necessary whenever the land is approached, and indeed the navigation of the British Channel, of the North Seas, of the Gulph of St. Laurence, and of the greater part of the coast of America, is chiefly carried on by means of soundings; in many cases they are of more consequence than either light-houses or beacons; even one head-land has been frequently mistaken for another, and the land fall, or gradual shallowing, can only be ascertained by correct and frequent soundings. Hence it is that men are perpetually relieving each other at the lead, as there is scarcely any other means of self-preservation, during fogs and hazy weather in close seas.

Soundings have been hitherto taken by means of a lead, to which is attached a line with marks on it denoting fathoms, which being heaved over-board, the length of line is supposed to give the depth of water. The fact would be correct were the ship stationary, and it has in consequence been found *necessary to bring the ship to* to take soundings above 12 fathoms, which appears to be about the limit at which they can be obtained, if the ship be going at the rate of five knots. When the ship is proceeding with a greater velocity,

velocity, or deeper soundings are required, it will be necessary to bring the ship to, which must be attended with much danger if in a gale, or in a convoy during foggy weather—and with much loss of time to the trader, who is running to procure a good market. These considerations have been the cause of a neglect, that has cost this country numbers of lives. But in a military point of view, where is the captain who would dare stop his ship in chase to sound? though on an unknown coast. Thus, under the present system of soundings he has but the option of risking his ship, or his reputation!

To many persons, who hear so much talk of longitude and improvements in navigation, it will be natural to suppose that the exact position of a ship is easily and generally known; but such persons are probably strangers to an act of parliament of the 14th of his present majesty's reign, which holds out a reward of

5000*l.*	to the first person who shall find the means of discovering longitude within	60 miles.
7000*l.*	....	40 do.
10,000*l.*	....	30 do.

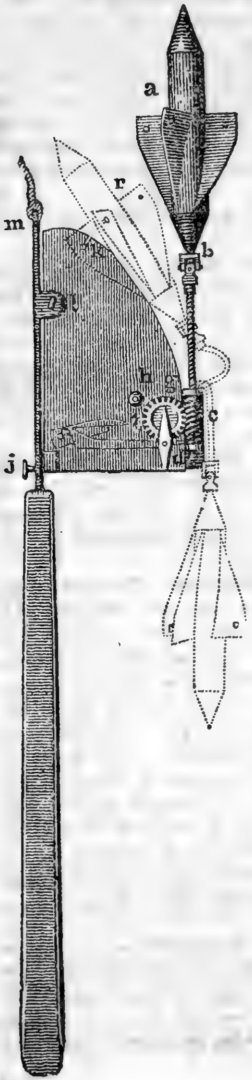
Nor is it an uncommon thing to have an error of from one to two hundred miles in a passage from America or the West Indies.

These considerations show how requisite it is to be able to take correct soundings at all times and in all weathers, under full sail or brought to; in chase or chased—to be able to employ any seaman, and not particular men, who have required a long training to the business,—to be able to repeat soundings often, without impeding the progress of the ship; for it frequently happens, and in the East Indies particularly, that a ship is carried away by currents from 40 to 100 miles, while the captain has perhaps no other means of ascertaining or correcting the position of the ship but by soundings.

Having said thus much on the general question of soundings, it will be necessary to describe the new sounding machine, to point out the severe trials it has been put to, and finally to show how far the invention has fulfilled the object for which it was invented.

The inventor is Edward Massey of Newcastle, in Staffordshire. He was bred to the trade of clock- and watchmaker under his father, and obtained two premiums from the Society of Arts, Manufactures, and Commerce; the one

one for a clock escapement, the other for the striking part of a clock*.



The machine consists of a rotator *a* formed of an air-tight tube, with four vanes solidly fixed to the tube, but inclined to the axis of it, which inclination causes the rotator to turn as it passes through the water, once in the space of five feet. Any number of rotators may be regulated to the 30th part of an inch. The rotator is attached to an endless screw *c*, (by means of an universal joint *b*, and a piece of line about three inches long) which acts in a wheel *g* of 24 teeth, the graduated scale on the wheel being of 20 parts to convert the rotations into fathoms. An index fixed to the brass plate points to the numbers on the wheel as it turns round. The axis of the wheel is a pinion of seven leaves, that turns another wheel on the reverse of the plate of 56 teeth answering to 160 fathoms. At *k* is a notch to hold the rotator in a horizontal position *r* by one of the vanes, until the machine strikes the water, which immediately forces the rotator into the perpendicular position *a*. The brass plate is fixed to an iron bar *mj*

* In volume xxi. of the Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce, a full description of these inventions may be found. Of the striking part of a clock, for which a premium of £21. was given, at page 402. And of the improvement in clock escapements, for which £50. was awarded, at page 407. Complete models of both are preserved in the Society's Repository.

about

about two feet long by means of the screw *l*, and by means of a prolongation of the brass plate at *j*, to which a nut is screwed on the opposite side of the bar. This prolongation acts as an axis or centre in the iron bar, to enable the brass plate to move between two small cheeks at *l*, the screw at *l* going through the two cheeks and plate at right angles with the surface of the plate, thus allowing the plate to move between the cheeks for the space of about an inch to obviate the eddy of the plate. The line is fixed at *m*, and the lead is cast on to the bar about an inch below *j*. The brass plate projects about six inches, lest the rotator should be affected by the eddy of the lead.

When the machine reaches the ground, or is attempted to be drawn up again, the rotator falls into the position below *c*, which prevents the rotary motion, but as an additional security a locking plate *d* is acted upon by a bolt which is fixed to and put in motion by the flirt piece *t*. During the descent, the flirt piece is kept in an upright position by the water, but ceases to be so when the machine becomes stationary or is hauled up.

Thus it appears that the whole of the apparatus, which really indicates the soundings, is comprized in the space of about one foot, that is to say, *from the summit of the rotator to the locking plate*, without any dependance on the length of the line or rate of sailing.

Such is the description of the machine. No sooner was it completed, than it was judged to be of sufficient correctness to stand the test of the severest trials. Testimonies are frequently obtained on very light grounds; but those obtained for this machine are of a very different stamp. Captain Malcolm, of the Royal Navy, having used Mr. Massey's log* during his passage to and from the West Indies, recommended the sounding machine to the attention of Admiral Montagu, who commanded at Portsmouth. The admiral ordered "the masters of His Majesty's ships the *Royal William* and *Resistance* to proceed off the Isle of Wight, for the purpose of trying the correctness of the machine;" and the joint report of the masters in date 20th June 1806, was, that "sailing at the rate of 5½ or 6 knots

* Capt. Malcolm, in a letter of the 12th of May 1806, says, "I used the Log Machine during my passage to and from the West Indies, and found it gave the ship's run perfectly correct." The captain had purchased it of Capt. Whittle, of the merchant service, who had carried the log out from Liverpool to Newfoundland, from thence made Cape St. Vincent, with an error only of *two miles*, and then proceeded to Naples, where he sold it to Capt. Malcolm.

they

they got true soundings in $17\frac{1}{2}$ fathoms of water, when by the hand-lead they could scarce get ground: the vessel was hove to immediately, and they sounded by a correct hand-lead and the machine, which agreed exactly. This operation was used several times, and not the smallest error found." Three days after Capt. Hope, of the *Espoir*, reported to Admiral Montagu as follows: "Yesterday having a fresh breeze, going at the rate of 8 knots, I obtained soundings frequently from 30 to 33 fathoms, with 55 fathoms of line out, exactly corresponding with the soundings laid down in the general charts." It is to be observed that Captain Hope adds in his letter to Mr. Massey, "at the same time the experiment was made with the deep sea-lead, and found impracticable, in consequence of the velocity with which the sloop was going through the water."

These official certificates were so strong, that the Navy Board judged it expedient to recommend that further experiments should be made on board the Channel fleet, then under the command of the Earl St. Vincent. His lordship when transmitting the reports he had received to the Board says, "I had intended to have made further trial of the sounding machine under the inventor's immediate inspection, in other of the detached squadrons from the fleet under my command; but the reports from Sir Charles Cotton are *so satisfactory* and *decided*, that unless you wish for further information, I do not propose any longer to detain that ingenious man from his family." On the 8th of January 1807, Sir Charles Cotton writes: "I have the honour to inclose for your lordships' information the several reports on Mr. Ed. Massey's Patent Sounding Machine, made to me by Captains Bedford (of the Prince of Wales), Lukin (of the Mars), and Neve (of the San Joseph); from all of which it appears to be an invention *of great utility in navigation.*" The following extracts from the reports of the above-mentioned captains must at once stamp the value of the discovery.

Capt. Bedford reports to Sir Charles, as follows:

"In compliance with your directions, I have caused repeated trials of Mr. Edward Massey's Sounding Machine, and, as far as I have seen, the construction and principle appear simple; and I think it a discovery of the greatest importance to navigation. The depth of water is easily and correctly ascertained (even if the ship be going six or seven knots, in sixty fathoms water) by affixing a second lead, and preventing the line from being checked; and should you not succeed in gaining bottom, you always ascertain the

the perpendicular descent of the lead, which in many cases (particularly approaching soundings in the English Channel) would be the greatest consolation of a dark night, when both the delay and danger attending it is often an objection to its being attempted in the present way. I am also of opinion, that in chase, or on a lee-shore, it might be of the greatest use, as no skill is necessary on the part of the person who takes the soundings, nor are the depth of water, the roughness of it, or the ship's going through it, interruptions to Mr. Massey's method of sounding."

Capt. Lukin writes:

"Agreeably to your orders, I have made repeated trials of Mr. Massey's Sounding Machine, and am enabled to say, that it gave the depth of water accurately, as far as our experiments went, up to the rate of six knots.

"We repeatedly got soundings in sixty fathoms, going at the rate of five knots and a half, without in the least deadening the ship's way, with the same accuracy that could have been done with a lead and line in that depth, had the ship been brought to."

Capt. Neve, after calling it a most excellent invention, proceeds to say:

"The invention is the more valuable, as the process is the most simple, the whole being understood by seeing it once in use.

"I therefore consider it a valuable improvement in navigation; as in frequent and various cases, soundings could not be gained without it. The advantages are many, such as in chase, or being chased; on a lee-shore, or doubtful of it; and to save time in running for the desired port."

These testimonies induced the Navy Board, in February 1807, to order 500 machines for the use of the Royal Navy, and about eighteen months after gave a further order for 250 more *. Thus was the machine introduced into the Royal Navy.

Every thing now appeared to be settled, nor was any objection to the new sounding machine ever hinted at, when Captains Hotham and Bedford discovered, that if soundings beyond 80 fathoms were attempted, the air-tubes of the rotators were frequently flattened, and that few of the rotators could withstand the pressure of 112 fathoms.

No sooner was this unexpected failure made known to the inventor, than it was instantly remedied by a small hole

* On the 28d of May, 1811, the Navy Board gave a further order for 1000 machines, making a total of 1750, for the use of the Royal Navy.

made in the bottom of the air tube, which restored the equilibrium between the interior and the exterior. But thus perforated, the rotary motion was true only in as much as the descent was not less than 17 fathoms per minute. To obviate this objection the hole was suppressed, and brass disks were introduced into the interior of the tube as a support, and they have rendered it proof against any pressure to which the machine has hitherto been exposed, as will be seen by the extracts from the following letters.

1. From Mr. Thomas Stokes, master of the fleet off Cadiz, May 31, 1810. He says, "I have had the opportunity of trying one of the rotators in a calm, and sunk it 180 fathoms, and it came up as perfect as when it was put in the water. I therefore congratulate you on having got over the only objection that seems at present to be against the sounding machine."

2. From Mr. Alexander Lumsdale, master of the Caledonia, 120 guns, September 7, 1810, who writes: "The late improvement you have made in the rotator, has made it a very complete thing. I have tried it in nearly 200 fathoms without its receiving the least injury, and am of opinion the great utility of the sounding machine only requires to be generally known, to make it more sought after."

3. From Mr. John Tapper, master of the Clyde, July 22, 1810, who says: "I have to acquaint you that I made frequent trials of your improved rotator in 200 fathoms with a lead of 14 lbs. attached to the proper lead * issued from the dock-yard in the manner Mr. Stokes requested, and found it equal to the pressure of the water with the above-mentioned depth and weight. It came up in perfect good state, not a drop of water had penetrated through it. My opinion is that no alteration is necessary."

He then proceeds on a subject that has not been mentioned in this Report, that is to say, *Surveying*, and he continues thus: "The machine is altogether an excellent invention, and I found it of very great use to me in sounding the river Tagus, where the water is deep, from 20 to 30 fathoms. In a boat with sails set, I could get true soundings in sailing to and fro, *but with the common lead, I should have been obliged to lower the sails, so as for the*

* It has been found more advantageous to fix the additional lead at 30 or 40 fathoms *above the machine*, by which means the buoyancy of the line is compensated, with less danger of the line breaking when the machine is hauled in.

boat's

boat's way to be entirely lost, to get true soundings, which enabled me to sound the river in less than half the time, *and with more accuracy.*" But as this letter only relates to the survey of a river, it will be proper to add the following extract from a letter of Mr. Nath. Denham, master of the Daphne, October 10, 1809. "Having been on a survey between the islands of Jersey and the rocks to the southward called the Minquies, as also ranging round the island of Jersey, and along the French coast, &c. by order of Rear-admiral D'Auvergne, &c. I used your valuable machine for soundings while running on rhumb lines from one head land to another on the opposite shore, such as from La Corberre Jersey, to the Isle of Brahat; from La Rochport to Choseys; from thence to Cape Frehel, &c. &c. by which means I was able to make a direct course, and *get perpendicular soundings without heaving to,* so essential to surveys, but saving also much time and trouble, &c."

Thus has the progress of the sounding machine been followed through all its stages; a machine calculated to preserve the lives and properties of thousands, to give ease and comfort to those heroes who devote themselves to the defence of their country. May it prove a source of emolument to the inventor! for, if his labours are to avert desolation from the families of Britons not only in the present but in future ages, his own family can never be neglected by a liberal and grateful nation.

The inventor has presented one of the machines to the Royal Institution, which is preserved in their repository by order of a General Meeting.

XIX. *On the Preparations of Gold lately employed medicinally.* By A. S. Duportal, *M.D. &c. and* Th. Pelletier, *Apothecary* *.

After having enjoyed some reputation as a medicine, gold had ceased to be administered to the patient, and taken an opposite direction. Lately, however, Dr. Chrestien, of Montpellier, a physician of great reputation and successful practice, has revived its use. He has employed it in siphylitic and lymphatic affections, and chiefly in Clark's mode. The preparations he uses are metallic gold in a state of minute division, oxide of gold precipitated by potash, the oxide precipitated by tin, and the triple muriate

* *Ann. de Chim.* vol. lxxviii. p. 38.

 of

of gold and soda. These he considers as superior to mercurials. Some experiments by Mr. Vauquelin, on the preparations of gold thus introduced into notice, have already been given: but we shall now present our readers with some remarks on the subject by the gentlemen above mentioned, one of whom enjoyed the advantage of a personal acquaintance with Dr. Chrestien at Montpellier.

The first preparation of gold employed by this physician was the metal in a state of minute division. To obtain this, he found an amalgam, by triturating leaf-gold with seven times its weight of mercury in a marble mortar with a glass pestle, and then expelling the mercury by means of a powerful lens in the height of summer, or dissolving it out by pure nitric acid.

The present writers recommend rather to precipitate a solution of muriate of gold by a solution of sulphate of iron at a minimum, filtering, and washing the precipitate with water, acidulated by muriatic acid, in order to dissolve out the oxide of iron mingled with the precipitated gold. When the gold is thoroughly dried, it is in the form of a deep brown powder, though in the metallic state; all metals losing their brilliancy by being minutely divided.

To prepare the oxide of gold precipitated by potash, they direct one part of nitric acid at 40° [sp. gr. 1·396] to be mixed with four of muriatic acid at 12° [1·089]; and cupelled gold to be heated with eight times its weight of this menstruum in a matrass with a long, narrow neck, till it boils gently. When no more gold will dissolve at this temperature, the solution is to be poured off, and evaporated to dryness in another matrass by a gentle fire. The residuum of this evaporation is to be dissolved in distilled water, and filtered.

The filtered solution is to be treated with potash, to separate from it the oxide of gold; but in this there are great difficulties, and the whole cannot be thrown down, without part of it being reduced to the metallic state. The cause of this is not known; but the authors ascribe it, 1. To the formation of a soluble triple muriate, which takes place when the potash is poured into the solution of muriate of gold: 2. To the excess of acid always present in this muriate: 3. To the more or less caustic state of the alkali employed: 4. To the greater or less quantity of this substance added to the muriate of gold.

When a solution of caustic potash is poured into a saturated

turated solution of gold by muriatic acid, a yellow precipitate is formed*, which, when collected on a filter, does not amount to more than 40 grs. of oxide from 72 grs. of the metal in the solution. The remaining liquid is of a very deep colour, and contains a triple muriate of gold and potash. A fresh quantity of the caustic alkali will cause no further precipitation, unless the liquid be kept several hours in a gentle heat: but in this case, a new precipitate will fall down, extremely bulky, and of a deeper colour than the former, and apparently at a different degree of oxidation. Several weeks are necessary to complete the precipitation; and even at last a certain portion of gold will remain, which must be thrown down by a slip of tin, if we would lose nothing.

If the solution of gold be very acid, there will be scarcely any perceptible precipitation; and this might be expected, as the alkali finds a sufficient quantity of free acid, to form muriate of potash enough for the production of the triple salt. Indeed, no precipitation at all ought to take place, when the solution is extremely acid; but here, experience does not entirely agree with theory, for a very small quantity of oxide of gold is always produced.

The causticity of the potash is of great importance; for if the neutral carbonate be employed, no change will take place without the assistance of heat. This, expelling a considerable portion of carbonic acid gas, will alter the colour of the solution from yellow to greenish. If it be then filtered, traces of the purple oxide of gold will be found; and it will effervesce with acids, having its fine golden colour restored. A few drops added to a glass of water will not colour it; but, if the water be acidulated, the colour will instantly appear. The same solution yields by evaporation white, transparent, alkaline crystals, interspersed with black spots. These crystals dissolve in water without colouring it; and on filtering the solution it passes transparent, leaving a little gold on the filter. The addition of any acid, however, causes its colour to re-appear.

What is the chemical nature of the crystals obtained? Though this was not minutely ascertained for want of time, it appears certain, that they were composed of carbonic and muriatic acid, potash, and gold; but whether constituting a quadruple salt, a triple, or two salts, one the triple muriate of gold and potash, the other subcarbonate

* It is necessary to employ heat.

of potash, the authors cannot say; nor could they form any judgement from the figure of the salt.

It may not be amiss to observe, that, in an impure nitromuriatic solution of gold, saturated carbonate of potash will precipitate the copper, without throwing down the gold, if no heat be employed.

As too large a quantity of alkali, added to a solution of muriate of gold, will cause a portion of the precipitated oxide to be redissolved, it is necessary to add the alkali cautiously, to boil the solution at every addition of alkali, and to separate the precipitate by filtration, whenever a sensible quantity appears.

The precipitate must be washed but slightly, it being partly soluble in water, as Mr. Vauquelin remarked; and it must be dried in the shade and in a cool place, otherwise it will be a mixture of oxide and metallic gold.

It may be known whether the oxide be pure, by treating it with muriatic acid, which in this case will dissolve it completely; but, if it be mixed with metallic gold, part will remain undissolved.

The oxide of gold precipitated by tin, which Dr. Chrestien also recommends, may be obtained either with metallic tin, or with its solution.

For the first, slips of tin well cleaned, are to be put into an aqueous solution of muriate of gold. These will soon be covered with a layer of pulverulent matter, of a colour more or less deep; which will be renewed several times, after being removed. When this ceases to be re-produced, the liquor is to be filtered, and the precipitate washed in distilled water, dried in the shade, and powdered. This is the purple powder of Cassius.

If the oxide of gold be precipitated by a solution of tin, it is of importance that the tin be in a fixed state of oxidation, otherwise the product will vary both in its nature and quantity. A uniform solution may always be obtained by dissolving slips of tin in muriatic acid at 12° [1·089], filtering, evaporating to the point of crystallization, dissolving the crystals in pure water, and filtering again. Part of this solution should immediately be mixed with the liquid muriate of gold; and the union of the two salts produces a precipitate, which should be increased by adding fresh quantities of the muriate of tin, as long as any thing is thrown down; after which the precipitate is to be washed, dried, and powdered. The quantity obtained, appears to depend on the quantity of water added to the solutions of gold and tin. The more they are diluted, the more the tin is thrown down.

down. One drachm of gold, the solution of which was mixed with ten quarts of water, mixed with a very dilute solution of tin, yielded five drachms and a half of a very fine purple precipitate.

It does not appear to be a matter of indifference which of these two precipitations is used. When metallic tin is employed, the precipitate is brown; and the gold, if not in the metallic state, is nearly approaching it. On the contrary, the precipitate produced by muriate of tin at a minimum of oxidation, is of a deep purple colour; and, though it contains a little metallic gold, has much more of the oxides of gold and of tin; whence, it is obvious, the efficacy of the two preparations cannot be the same.

The muriate of gold is so greedy of moisture, that it soon deliquesces: whence it can be employed only in the liquid state; and, as its great causticity renders even this difficult, Dr. Chrestien thought of uniting it with the muriate of soda; thus producing a triple muriate, less deliquescent, and less caustic.

For this purpose, a solution of muriate of gold in distilled water, obtained as described above, is to be employed; and it is particularly important that this salt has not an excess of acid. Into this solution is to be poured an aqueous solution of pure decrepitated muriate of soda, so as to combine an equal quantity of the dry salt with the gold dissolved. The two solutions being mixed, the fluid is to be evaporated by a gentle heat in a glass capsule, taking care to stir it well toward the end of the process. When the mass is sufficiently dry, it is to be powdered while hot in a glass or stone mortar; and the powder is to be kept from moisture, which it attracts in a slight degree.

In this preparation the management of the fire is of great importance: for, if the desiccation of the salt be not carried far enough, it will contain too much acid; and, if it be urged too far, it will be in part decomposed, and mixed with a little gold.

The enlightened physician who extols the use of these preparations, employs them externally and internally; but recommends them to be mixed with other substances, lest their action should be too violent, if given alone. Thus for a long time he did not give the triple muriate of gold and soda, otherwise than mixed with twice its weight of a powder composed of starch, charcoal, and the lake used by painters. As the alumine of the last, however, might take up a portion of the muriatic acid, and the charcoal might revive the gold, Dr. Chrestien changed this powder for liquorice root, orris root, &c.

Beside this, he joined the compounds of gold with extracts of the attenuant plants; sugar, with which he forms lozenges; syrups, in which he dissolves them, &c. He mixes them also with Galen's cerate, when he wishes to promote suppuration; and with lard, when he would employ them in frictions on the soles of the feet, after the method of Cyrillo.

The writers of the present article do not approve of the combination of the preparations of gold with these different substances, as all vegetable and animal substances, dissolved or not, revive gold from its acid solution. They recommend them to be given alone, or dissolved in distilled water: or, at least, if they must be mixed, to mix them as short a time as possible before they are used.

In this way Dr. Duportal asserts that he has found good effects from them in siphylitic complaints. In a chancre corroding one of the corpora cavernosa he found them of real advantage: but the most striking instance of their efficacy was in a cancerous ulcer, that had destroyed the upper lip, attacked the soft parts of the nose and left cheek, destroyed the square bones [*os carrés*], and rendered the maxillary bone carious. Being called to a consultation with Dr. Payen on this very serious case, in which all the common methods had been tried in vain, Dr. Duportal hoped to oppose the progress of the disease by the use of Dr. Chrestien's medicine, assisted by attenuant extracts. In consequence, the patient was directed daily to rub into the gums the triple muriate of gold and soda; and to take oxide of gold precipitated by potash, with pills composed of the extracts of white henbane, hemlock, and sharp-pointed toad-flax. The ulcer was daily washed with Sydenham's liquid laudanum, sprinkled over with powder of red bark and camphor, and dressed with a digestive, in which oxide of gold was mixed. Under this treatment, which has been continued two months, gradually increasing the dose of the substances, the ulcer has assumed a promising appearance; the carious points have disappeared; the suppuration furnishes laudable pus in moderate quantity; the patient daily improves in flesh and strength; and there is every reason to believe that this evident melioration will continue. That it cannot be ascribed to the means employed in conjunction with the preparations of gold is evident, for they had been used previous to these, without effect.

XX. *Mr.*

XX. *Mr.* Hume's *Remarks on his Test for Arsenic.*

To Mr. Tilloch.

Sir, The great importance of a sure method to detect arsenic, whether the question be considered judicially or chemically, induces me once more to address you upon this subject, particularly as the test, originally discovered by myself, is now clearly demonstrated to surpass all others in precision and efficacy; for, such is its power that, by proper management, it will prove the existence of white arsenic, even if dissolved in more than *four hundred thousand times* its weight of water, when, probably, in such a rare state of dilution, this dreadful body ceases to be a direct poison.

In my letter to you, inserted in the xxxiiid volume of your excellent Magazine, and in two other letters published afterwards in the "Medical and Physical Journal," dated May and October 1810, I have, in general terms, described the best process for discovering the arsenic to be, that of *combining it with silver*. To effect this, I advised that the arsenic should be nearly saturated with any alkali; and that nitrate of silver, the common lunar caustic, should then be applied to the surface of the solution in which the poison is suspected to exist;—if a bright *yellow* colour appear on presenting the nitrate of silver, we may conclude without reserve, that some of this poison is in the mixture under examination.

By the three letters just quoted, it will be seen that this plan is not confined to any particular alkali, as the sole use of such addition is merely to neutralize the arsenic, so that the nitrate, or indeed any other salt of silver, may be more readily decomposed, and beyond this point the quantity of alkali should not pass.

But, lest any one disapprove of this scheme, urging the difficulty of assigning the *quantity* of alkali requisite for the point of saturation, I shall give, in the present communication, another prescription, which has so completely succeeded with me that, I presume, it must now supersede all other tests for arsenic, and become the standard to future operators. The name which I shall beg to offer for this test is, *ammoniaco-nitrate of silver;* and though the proportion of the water to the other ingredients is of little consequence, still the liquor when finished will always be sufficiently uniform, and quite free from any imperfection whatever, provided the articles themselves be pure.

Dissolve

Dissolve a few grains, say ten, of the nitrate of silver called lunar caustic, in about nine or ten times its weight of distilled water; to this add, by a drop at a time, a solution of ammonia till a precipitate is formed. Continue to add the ammonia, now and then shaking the bottle, till the precipitate shall be taken up and the solution again become transparent, or nearly so, as the ammonia need not be in great excess, if in any; for, solution of ammonia being lighter than water, the superfluous portion would remain on the surface of any fluid to which this test-liquor may be applied,—a circumstance not noticed by other analysts.

Here we have one simple liquid, which, if kept in a phial with a glass stopper, will not easily spoil, and may be always at hand. Its application is also equally simple; for nothing more is required than to dip a piece of glass into this liquor, and apply it to the solution containing arsenic. Should the material suspected to contain arsenic be of a dry nature, such as a mixture of sugar, meal, bread, meat, or any other kind of food, let some boiling water be poured upon the suspected substance, and filtrate the solution through paper. The strip of glass may be procured at any glazier's shop; but, if not at hand, a few drops of the test from the phial may be put in, as there is here not so much uncertainty from an excess of alkali, nor even of the test-liquor itself.

In the 2d volume of "Medico-chirurgical Transactions," there is a "Case of Recovery from Arsenic," by Dr. Roget, in which my test of silver was employed with great advantage. The whole chemical part of this paper is, however, described in such a way, that whoever reads the "case" would readily believe that Dr. Marcet, and not I, had originally projected this most delicate test; especially when my pretensions are so pointedly censured and slurred over by Dr. R. who has indeed condescended to quote, though not accurately, my letter to you.

In that paper Dr. Roget has ushered in my own process for detecting arsenic with *ammonia* and nitrate of silver; but this is described as altogether a *new* test, to which no reader would suppose I had the least claim. On the present occasion I shall make but few remarks upon the injustice done me by this gentleman's publication; but it is evident that my own observations upon the test by means of silver, are placed in the Medico-chirurgical Transactions as original matter; as well as the remarks I made on some of the peculiar and reciprocal habitudes of white oxide of arsenic, the arsenical acid, and nitrate of silver, which are all derived from the

three

three letters, only one of which has been quoted by Dr. R. Many of my expressions and even words have been warped from their proper authority without a fair acknowledgement of the source; I need mention but one instance here, and that is, where I have pointed out the difference between the *arsenite* and *arseniate* of silver; the first, as a beautiful *yellow*; the second, as a precipitate of a *red-brick* colour.

These gentlemen seem to have taken the same quantity of ammonia as well as of the solution of silver in every case; for they used apparently the same glass-rods to detect the *tenth* as for the *fifty thousandth* part, of a grain of arsenic. The diameters of these rods were, we must believe, always alike, as no caution on this score was observed by Dr. Marcet, to whom the chemical part is chiefly ascribed; therefore, unless the two solutions be proportioned to each other in point of strength and dilution, the application of the test in the way Dr. R. has recommended, must be fortuitous if not erroneous, otherwise the caution given, (p. 157,) that "the quantity of either ammonia or nitrate of silver employed, can scarcely be too small," is quite insignificant.

Why two glass-rods were employed when one must necessarily have *two* ends, I cannot explain; nor can I conceive why a glass-rod should be used for the ammonia, and a *clean* glass-rod for the solution of silver:—such niceties are surely unnecessary to the success of the experiment; I would rather have inculcated more attention to be paid to the solution of silver, which should always be prepared with the *fused* nitrate, so that there may be no excess of acid.

It is in all cases material to know in what quantity of water the fractional part of a grain of white arsenic is dissolved; for this circumstance is of great moment in making comparative estimates of the powers of chemical tests, especially one of such uncommon delicacy and security as that now under consideration,—the arresting of a mere invisible atom of white oxide of arsenic by means of silver, so as to make it obvious to our sense.

Having brought my test to such perfection, it is proper that I should give you some account of the methods I pursued in the investigation, that other practitioners may be enabled to apply its powers with the same success.

The first solution, with which I made the experiments, was prepared in this way. One grain by weight of white oxide of arsenic in powder, together with 4999 grains of distilled water, were introduced into a glass matrass. By the

the application of a spirit lamp the solution was made to boil, when the oxide became entirely dissolved. As the vessel with its contents had been previously weighed, any deficiency by the operation of boiling was easily remedied, by adding to the fluid, when cold, more distilled water to complete the 5000 grains, being the original weight of the solution.

Fifty grains of this solution, containing $\frac{1}{100}$th of a grain of arsenic, were further diluted with 500 grains of distilled water, and to this the test-liquor was applied by means of a small strip of glass. Another 50 grains of the solution was treated in the same way with 1000;—a third with 1500, and so on, progressively, in the same ratio, until I had arrived at 5000 grains of the distilled water. At this point I deemed it but fair to stop, being perfectly assured of the efficacy of the test in any case that can possibly require its aid, since the proportion of the arsenic to the water was in the last experiment as about *one* to 500,000, a degree of nicety to which I had never expected to attain.

I must observe, that, in proportion to the degree of dilution more or less time should be allowed for the effect to become perceptible; for, in the last experiment the test did not act till some minutes had elapsed, and then the fluid became slightly tinged without losing its transparency, appearing more like very dilute white wine and water. In the course of some hours, perhaps 12, an evident *dark-coloured* precipitate had assembled at the bottom, a sure indication of the presence of arsenic; for all these precipitates, even the beautiful yellow, lose their first colour and become dark; setting aside all hopes of our converting them into pigments.

I prefer cylindrical glass-vessels to watch-glasses for such experiments; and where very minute portions of arsenic are to be made manifest by these means, it is prudent to place a similar vessel, filled with distilled water, near to the first, so that by contrasting the contents of these two vessels the slightest change will be more conspicuous.

I have now, it is presumed, confirmed my claim to *the arsenical test by means of silver*, and brought into notice what must be considered as an acceptable improvement in chemistry and its dependent science, metallurgy: it will likewise afford to British juries a more sure guide to their decisions, in all such criminal cases where the existence of arsenic must be ascertained, by calling for the evidence of this test; and hence, the laws of our country in the administration

ministration of public justice will be aided, and the verdicts upon these unfortunate cases, rendered quite unexceptionable by this discovery.

I remain, sir,

Your obedient servant,

Long Acre, Aug. 10, 1812. JOS. HUME.

XXI. *Chemical Researches on the Blood, and some other Animal Fluids. By* WILLIAM THOMAS BRANDE, *Esq. F.R.S. Communicated to the Society for the Improvement of Animal Chemistry, and by them to the Royal Society*.*

SECTION I.

Introduction.

IN the following pages I shall have the honour of laying before this Society an account of some experiments upon the blood, which were originally undertaken with a view to ascertain the nature of its colouring matter. The difficulties attendant on the analysis of animal substances have rendered some of the results less decisive than I could have wished, but I trust that the general conclusions to which they lead, will be deemed of sufficient importance to occupy the time of this body.

The existence of iron in the blood was first noticed by Menghini †, and its peculiar red colour has been more recently attributed to a combination of that metal with phosphoric acid, by MM. Fourcroy and Vauquelin ‡. The very slight discoloration occasioned by the addition of infusion of galls to a solution of the colouring matter, under circumstances most favourable to the action of that delicate test of iron, first led me to doubt the inferences of these able chemists, and subsequent experiments upon the combinations to which they allude, tended to confirm my suspicion, and induced me to give up no inconsiderable portion of the time which has elapsed since the last meeting of this Society, to the present investigation.

An examination of the chyle and of lymph, in order to compare their composition with that of the blood, formed an important part of this inquiry, especially as those fluids

* From the Philosophical Transactions for 1812, part i.

† Viucentius Menghinus de Ferrearum Particularum Progressu in Sanguinem. *Comment. Acad. Bonon.* t. ii. p. 2, pag. 475.

‡ *Systême des Conn. Chym.* vol. viii.

have

have not hitherto been submitted to any accurate analysis, on account of the difficulty of procuring them in sufficient quantities, and in a state of purity. Whilst engaged in assisting Mr. Home in his physiological researches, several opportunities occurred of collecting the contents of the thoracic duct under various circumstances, and in different animals; on other occasions Mr. Brodie has kindly furnished me with the materials for experiment.

SECTION II.

On the Composition of Chyle.

The contents of the thoracic duct are subject to much variation. About four hours after an animal has taken food, provided digestion has not been interrupted, the fluid in the duct may be regarded as pure chyle; it is seen entering by the lacteals in considerable abundance, and is of an uniform whiteness throughout. At longer periods after a meal, the quantity of chyle begins to diminish, the appearance of the fluid in the duct is similar to that of milk and water; and lastly, where the animal has fasted for twenty-four hours or longer, the thoracic duct contains a transparent fluid which is pure lymph.

A. The chyle has the following properties.

1. When collected without any admixture of blood, it is an opake fluid of a perfectly white colour, without smell, and having a slightly salt taste, accompanied by a degree of sweetness.

2. The colour of litmus is not affected by it, nor that of paper stained with turmeric, but it slowly changes the blue colour of infusion of violets to green.

3. Its specific gravity is somewhat greater than that of water, but less than that of blood; this, however, is probably liable to much variation.

4. In about ten minutes after it is removed from the duct, it assumes the appearance of a stiff jelly, which in the course of twenty-four hours gradually separates into two parts, producing a firm and contracted coagulum, surrounded by a transparent colourless fluid. These spontaneous changes, which I have observed in every instance where the chyle was examined at a proper period after taking food, are very similar to the coagulation of the blood and its subsequent separation into serum and crassamentum; they are also retarded and accelerated by similar means.

B. 1. The coagulated portion bears a nearer resemblance to the caseous part of milk than to the fibrine of the blood.

It

2. It is rapidly dissolved by the caustic and subcarbonated alkalies. With solutions of potash and soda, it forms pale brown compounds, from which, when recent, a little ammonia is evolved. In liquid ammonia the solution is of a reddish hue.

3. The action of the acids upon these different compounds is attended with nearly similar phænomena, a substance being separated intermediate in its properties between fat and albumen. Nitric acid added in excess redissolves this precipitate in the cold, and sulphuric, muriatic, and acetic acids when boiled upon it for a short time.

4. Neither alcohol nor ether exerts any action upon the coagulum of chyle; but of the precipitate from its alkaline solution they dissolve a small portion, which has the properties of spermaceti: the remainder is coagulated albumen.

5. Sulphuric acid very readily dissolves this coagulum, even when diluted with its weight of water; and with the assistance of heat, it is soluble in a mixture of one part by weight of acid, with four of water; but when the proportion of water is increased to six parts, the dilute acid exerts no action upon it. I was surprised to find that the alkalies produced no precipitation in these sulphuric solutions when heat had been employed in their formation, and where a small proportion only of the coagulum had been dissolved, and was therefore led to examine more particularly the changes which the coagulum had undergone by the action of the acid.

On evaporating a solution of one drachm of the coagulum in two ounces of dilute sulphuric acid (consisting of one part by weight of acid with three of water) down to one ounce, a small quantity of carbonaceous matter separated, and the solution had the following properties.

It was transparent, and of a pale brown colour.

Neither the caustic nor carbonated alkalies produced in it any precipitation, when added to exact saturation of the acid, or in excess.

Infusion of galls, and other solutions containing tannin, rendered the acid solution turbid, and produced a more copious precipitation in that which had been neutralized by the addition of alkalies.

When evaporated to dryness, carbonaceous matter was deposited, and sulphurous acid evolved, with the other usual products of these decompositions.

6. On digesting the coagulum in dilute nitric acid, consisting of one part by weight of the acid to fifteen of water, it was speedily rendered of a deep brown colour, but no other

other apparent change was produced for some weeks; when on removing it from the acid at the end of that period, it had acquired the properties of that modification of fat which is described by Fourcroy under the name of adepocire*.

A mixture of one part of nitric acid with three of water, acted more rapidly upon the coagulum of chyle; a portion of it was dissolved, and when the acid was carefully decanted from the remainder, it was found to possess the properties of gelatine. But when heat was applied, or when a stronger acid was employed, the action became more violent, nitrogen and nitric oxide gas were evolved, and a portion of carbonic acid and of oxalic acid were produced.

7. Muriatic acid in its undiluted state does not dissolve the coagulum of chyle; but when mixed with an equal quantity of water, or even more largely diluted, it dissolves it with facility, forming a straw-coloured solution, which is rendered turbid when the alkalies are added to exact saturation, but no precipitate falls, nor can any be collected by filtration. When either acid or alkali is in excess in this solution, it remains transparent.

8. Acetic acid dissolves a small portion of the coagulum of chyle, when boiled upon it for some hours. As the solution cools, it deposits white flakes, which have the properties of coagulated albumen.

9. The action of oxalic acid is nearly similar to that of the acetic, but neither citric nor tartaric acid exert any action upon this coagulum.

10. The destructive distillation of this substance affords water slightly impregnated with carbonate of ammonia, a small quantity of thin fetid oil and carbonic acid and carburetted hydrogen gas.

The coal which remains in the retort is of difficult incineration; it contains a considerable portion of muriat of soda and of phosphat of lime, and yields very slight traces of iron.

C. 1. The serous part of the chyle becomes slightly turbid when heated, and deposits flakes of albumen.

2. If after the separation of this substance the fluid be evaporated to half its original bulk, at a temperature not exceeding 200° Fahrenheit; small crystals separate on cooling, which, as far as I have been able to ascertain, bear a strong resemblance to sugar of milk: they require for solution

* *Mem. de l'Acad. des Sciences*, 1789.

lution about four parts of boiling water, and from sixteen to twenty parts of water of the temperature of 60°. They are sparingly soluble in boiling alcohol, but again deposited as the solution cools. At common temperatures alcohol exerts no action upon them. The taste of their aqueous solution is extremely sweet. By nitric acid they are converted into a white powder of very small solubility, and having the properties of saccholactic acid, as described by Scheele *.

The form of the crystals I could not accurately ascertain even with the help of considerable magnifiers. In one instance they appeared oblique six-sided prisms, but their terminations were indistinct.

Some of the crystals heated upon a piece of platina in the flame of a spirit lamp, fused, exhaled an odour similar to that of sugar of milk, and burnt away without leaving the smallest perceptible residuum.

3. The destructive distillation of the serous part of chyle afforded a minute quantity of charcoal, with traces of phosphate of lime and of muriate of soda and carbonate of soda.

SECTION III.

Analysis of Lymph.

The fluid found in the thoracic duct of animals that have been kept for twenty-four hours without food, is perfectly transparent and colourless, and seems to differ in no respect from that which is contained in the lymphatic vessels. It may therefore be regarded as pure lymph.

It has the following properties †:

1. It is miscible in every proportion with water.

2. It produces no change in vegetable colours.

3. It is neither coagulated by heat, nor acids, nor alcohol, but is generally rendered slightly turbid by the last reagent.

4. When evaporated to dryness, the residuum is very small in quantity, and slightly affects the colour of violet paper, changing it to green.

5. By incineration in a platina crucible the residuum is found to contain a minute portion of muriate of soda; but I could not discover in it the slightest indications of iron.

6. In the examination of this fluid, I availed myself with some advantage of those modes of electro-chemical analysis,

* Chemical Essays, No. xvii.

† The term lymph has been applied indiscriminately to the tears, to the matter of encysted dropsy, and to some other animal fluids. Vide Aikin's Dictionary of Chemistry and Mineralogy, art. *Lymph.*

lysis, which on a former occasion I have described to this Society *.

When the lymph was submitted to the electrical action of a battery consisting of twenty pairs of four-inch plates of copper and zinc, there was an evolution of alkaline matter at the negative surface, and portions of coagulated albumen were separated. As far as the small quantities on which I operated enabled me to ascertain, muriatic acid only was evolved at the positive surface.

SECTION IV.

Some Remarks on the Analysis of the Serum of Blood.

This fluid has been so frequently and fully examined by chemists, that I shall not enter into a detailed account of its composition, but merely state such circumstances respecting it as relate particularly to the present inquiry, and have not hitherto been noticed by the experimentalists to whom I have alluded.

The fluid which oozes from serum that has been coagulated by heat, and which by physiologists is termed *serosity*, is usually regarded as consisting of gelatine, with some uncombined soda, and minute portions of saline substances, such as muriate of soda and of potash, and phosphate of lime, and of ammonia. Dr. Bostock regards it as mucus †.

From some experiments which I made upon the serum of blood, on a former occasion, I was induced to regard the serosity as a compound of albumen with excess of alkali, and to consider the coagulation of the serum analogous to that of the white of egg, and of the other varieties of liquid albumen.

To ascertain this point, and to discover whether gelatine exists in the serum, I instituted the following experiments.

Two fluid ounces of pure serum were heated in a water bath until perfectly coagulated: the coagulum, cut into pieces, was digested for some hours in four fluid ounces of distilled water, which was afterwards separated by means of a filter.

The clear liquor reddened turmeric paper, and afforded a copious precipitation on the addition of infusion of galls, and when evaporated to half an ounce it gelatinized on cooling. It was rendered very slightly turbid by the addi-

* Phil. Trans. 1809, p. 373.

† Transactions of the Medical and Chirurgical Society of London, vol. i. p. 73.

tion

tion of dilute sulphuric and muriatic acid; but alcohol produced no effect.

From the result of these trials, it might have been concluded that gelatine was taken up by the water; but as an alkaline solution of albumen forms an imperfect jelly when duly concentrated, and as albumen and gelatine are both precipitated by tannin, I was inclined to put little reliance on the appearances just described, until I had examined the solution by the more accurate method of electrical decomposition.

Upon placing it in the Voltaic circuit my suspicions were justified, by the rapid coagulation which took place in contact with the negative wire. I therefore made some other experiments in order to corroborate this result.

One fluid ounce of pure serum was dissolved in three of distilled water: the conductors from a battery of thirty pairs of four-inch plates were immersed in this solution at a distance of two inches from each other; the electrization was continued during three hours and a half, the solid albumen being occasionally removed: at the end of that period, no further coagulation took place, and a mere decomposition of the water was going on.

Having ascertained in previous researches, that gelatine is not altered during the electrical decomposition of its solution carried on as just described, my object in this experiment was, to ascertain whether any gelatine remained after the complete separation of the albumen had been effected. I accordingly examined the water from which the coagulated albumen had been removed, and found that it was not altered by infusion of galls, nor did it afford any gelatine when evaporated to dryness.

Two fluid ounces of dilute muriatic acid were added to one of serum. The mixture immediately assumed a gelatinous appearance; it was heated, and a more perfect coagulation of the albumen took place; the liquid part was separated by a filter. No effect was produced upon it by Voltaic electricity, nor did infusion of galls occasion any precipitation.

I repeated the first experiment with the addition of twenty drops of a solution of isinglass to the serum. The liquid which now separated, after the albumen had been entirely coagulated by the action of electricity, was copiously precipitated by infusion of galls.

It may be inferred from these experiments, that gelatine does not exist in the serum of the blood, and that the serosity consists of albumen in combination with a large pro-

 portion

portion of alkali, which modifies the action of the re-agents commonly employed, but which is readily separated by electrical decomposition.

To ascertain whether iron exists in the serum of the blood, one pint was evaporated to dryness in a crucible, and gradually reduced to a coal, which was incinerated and digested in muriatic acid, to which a few drops of nitric acid were added; some particles of charcoal remained undissolved; the solution was saturated with ammonia, which afforded a copious precipitation of phosphate of lime, accompanied with slight traces only of oxide of iron.

SECTION V.

Some Experiments upon the Coagulum of Blood.

Mr. Hatchett's valuable researches on the chemical constitution of the varieties of coagulated albumen, have shown that that substance varies but little in its properties, whether obtained from the crassamentum of the blood, or from washed muscular fibre, or other sources; but that the proportion of earthy and saline matter is different in the different varieties*.

It will also be remarked, on referring to the dissertation which I have just quoted, that the ashes obtained by incinerating the coal left after the destructive distillation of albumen, did not contain any appreciable proportion of iron.

Assuming the existence of iron in the colouring matter of the blood, I made the following experiments upon the crassamentum of that fluid.

Two pints of blood were collected in separate vessels. The one portion was allowed to coagulate spontaneously; the other was stirred for half an hour with a piece of wood, so as to collect the coagulum, but to diffuse the principal part of the colouring matter through the serum. These two portions of coagulum were now dried in a water-bath, and equal weights of each reduced in a platina crucible to the state of coal, which afterwards was incinerated. The ashes were digested in dilute nitro-muriatic acid, and the solution saturated with liquid ammonia, in order to precipitate the phosphate of lime as well as any iron which might have been present.

The precipitates were collected, dried, and treated with dilute acetic acid, by which they were almost entirely dissolved, some very minute traces only of red oxide of iron remaining, the quantity of which was similar in both cases, and so small as nearly to have escaped observation.

* Phil. Trans. 1800, p. 384.

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It is reasonable to infer, if the colouring matter of the blood were constituted by iron in any state of combination, that a larger relative proportion of that metal would have been discoverable in the former than in the latter coagulum; but frequent repetitions of these experiments have shown that this is not the case, and the following result appears to complete the evidence on this subject.

The colouring matter of a pint of blood was diffused by agitation through the serum, from which it was allowed gradually to subside, the coagulum having been removed: after twenty-four hours, the clear serum was decanted off, and the remainder, containing the colouring matter, after having been evaporated to dryness, was incinerated, and the ash examined as in former experiments. But the traces of iron were here as indistinct as in the other instances above mentioned, although a considerable quantity of the colouring matter had been employed.

The minutiæ of analysis I have purposely excluded, as leading into details which would exceed the proper limits of this paper, and unnecessary in the present investigation. I shall now merely dwell on the principal results which have been obtained, and on the general conclusions which these afford.

[To be continued.]

XXII. *An Attempt to determine the relative Quantities of the constituent Parts of artificial Carbonate of Lime. By M.* J. Berzelius.

Bucholz found that several fossil carbonates of lime, as calcareous spar, chalk, &c. contained 56½ parts of lime, 43 parts carbonic acid, half part water *. But Klaproth says, that both the natural and artificial carbonates of lime consist of 45 parts carbonic acid and 55 parts lime†, from which the latter in several of his analyses calculates the quantity of lime in calcareous carbonates.

Though I had no reason to question the correctness of Bucholz's account, it occurred to me that the artificial carbonate of lime (of which he had not communicated any separate examination) might possibly approximate the statement of Klaproth; and as it was requisite that I should know this to a certainty, for the sake of several other analytical experiments, I determined to investigate the composition of this compound with the utmost exactness.

* *Neues Algem. Journal der Chemie,* vol iv, page 410.
† *Klaproth's Beytrage,* vol. iv. page 210.

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I precipitated a muriatic solution of pure carbonate of lime with a very pure neutral alkaline carbonate. The deposit was voluminous, and took place without effervescence; but having stood for a few minutes on a warm place, a strong disengagement of carbonic acid ensued, and the precipitated carbonate of lime fell to the bottom in a ponderous powder, leaving the fluid clear. It therefore appeared that the calcareous earth in the moment of precipitation was capable of attracting more carbonic acid than it could afterwards retain, at the common temperature of the atmosphere. The precipitate, placed on a filter, was washed as long as the percolated liquor appeared to be acted upon by a solution of silver, and was dried at a heat exceeding + 100°, but which nevertheless did not scorch the paper.

1. Five grammes of this carbonate of lime were poured by small portions into a glass phial containing nitric acid, the weight of which was previously well ascertained. The phial during the solution was kept in a slanting position, so that nothing could be thrown out by the effervescence. After a complete dissolution, the mass was gently shook in the phial for a few minutes, to favour the evolution of such carbonic acid gas as might possibly remain in the fluid. The mass had lost 2·18 grains, which, when deducted from 5 grains, give 43·6 per cent. carbonic acid.

2. Five grains of the same carbonate of lime were ignited in a covered platina crucible in a small furnace for two hours, by which the mass had lost only 1·53 grains, or 30·6 per cent. When afterwards dissolved in nitric acid, it further lost 0·65 grains, or precisely as much as in the preceding experiment.

3. Displeased with the less favourable result of the latter attempt, I weighed an ignited Hessian crucible, and put in 5 grains of the same carbonate of lime. Covered with a close fitting crucible, it was ignited in a furnace for one hour, and was found to have lost 1·7 grains, or 37 per cent. of the weight of the mass. It was then exposed for one hour to the strongest heat that could be produced in an air furnace, and had on cooling lost 2·18 grains, or exactly as in the previous instances. The remaining 2·82 grains of lime earth correspond to 56·4 per cent. of the weight of the carbonate of lime. I slacked it with water, whereby it heated much and fell asunder, but dissolved thereafter in nitric acid, almost in a moment, without any discoverable sign of hissing, and without any loss of weight.

Bucholz had found 43 per cent. acid and ½ per cent. water. Did then this ½ per cent. escape in my experiment (three

(three times repeated) evaporated in the carbonic acid gas? or, in Bucholz's case, might not a small portion of carbonic acid gas have remained in the liquor? As Bucholz is one of our most accurate chemists, and therefore I suspected a mistake in my own experiment:

To ascertain the point, I dissolved in nitric acid in a weighed apparatus 5 grains of white marble coarsely bruised. The gas was conducted through a glass tube, also weighed, and filled with dry muriate of lime kept cold by snow placed around it. The pieces of marble were dissolved in two hours. The tube had gained so trifling an addition of weight, that I have great reason to believe that the increase was derived from a particle of water that had spurted into it during the dissolution. The total loss was 2·182 grains. The carbonic acid gas had thus departed without any hygrometric or evaporated water; for it did not deposit any in the muriate of lime. But it might be objected that when the globules of gas are surrounded by the new formed salt and the acid, which strongly retain the water, then all evaporation is prevented. I therefore repeated the experiment with the acid diluted by double its weight of water, and dissolved 5 grains of marble therein. The tube now gained during the passage of the gas 0·05 grain, and the vessel lost 2·175 grains; which, deducting the 0·05, leaves 2·125. I weighed the glass immediately on taking it up, and then shook it gently for a few minutes, during which it again lost 0·062; so that its total loss became 2·187, that is 0·007 more than in the foregoing trials. It did not lose any further weight in seven hours, slightly covered with paper in + 5° temperature. This experiment shows, that when the nitric acid is too much diluted, the accuracy of the result may be affected in two ways; partly by water evaporating along with the gas, and increasing its weight, and partly by a portion of the gas remaining dissolved in the diluted fluid, and carrying with it a further portion of water in its escape.

I conclude from these experiments, that carbonate of lime, as well the natural as the artificial, consists of 56·4 of lime, and 43·6 of carbonic acid, and that it does not hold any water of crystallization.

XXIII. *Some Particulars respecting the arithmetical Powers of* Zerah Colburn, *a Child under Eight Years of Age.*

London, August 20, 1812.

The attention of the philosophical world has been lately attracted by the most singular phænomenon in the history

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of the human mind that perhaps ever existed. It is the case of a child, *under eight years of age*, who, without any *previous* knowledge of the common rules of arithmetic, or even of the *use and power* of the Arabic numerals, and without having given any particular attention to the subject, possesses (as if by intuition) the singular faculty of solving a great variety of arithmetical questions *by the mere operation of the mind*, and without the usual assistance of any visible symbol or contrivance.

The name of this child is Zerah Colburn, who was born at *Cabut* (a town lying at the head of Onion river, in Vermont, in the United States of America,) on the 1st of September 1804. About two years ago (August 1810) although at that time *not six years of age*, he *first* began to show those wonderful powers of calculation which have since so much attracted the attention and excited the astonishment of every person who has witnessed his extraordinary abilities. The discovery was made by accident. His father, who had not given him any other instruction than such as was to be obtained at a small school established in that unfrequented and remote part of the country, (and which did not include either *writing or cyphering*,) was much surprised one day to hear him repeating the products of several numbers. Struck with amazement at the circumstance, he proposed a variety of arithmetical questions to him, all of which the child solved with remarkable facility and correctness. The news of this *infant prodigy* soon circulated through the neighbourhood; and many persons came from distant parts to witness so singular a circumstance. The father, encouraged by the unanimous opinion of all who came to see him, was induced to undertake, with this child, the tour of the United States. They were every where received with the most flattering expressions; and in the several towns which they visited, various plans were suggested to educate and bring up the child, free from all expense to his family. Yielding, however, to the pressing solicitations of his friends, and urged by the most respectable and powerful recommendations, as well as by a view to his son's more complete education, the father has brought the child to this country, where they arrived on the 12th of May last: and the inhabitants of this metropolis have for these last three months had an opportunity of seeing and examining this wonderful phænomenon *, and of verifying the reports that have been circulated respecting him.

* At the Exhibition Rooms, Spring Gardens.

Many

Many persons of the first eminence for their knowledge in mathematics, and well known for their philosophical inquiries, have made a point of seeing and conversing with him; and they have all been struck with astonishment at his extraordinary powers. It is correctly true, as stated of him, that—" He will not only determine, with the greatest " facility and dispatch, the exact number of *minutes* or *se-* " *conds* in any given period of time; but will also solve any " other question of a similar kind. He will tell the exact " *product* arising from the multiplication of any number, " consisting of two, three, or four figures, by any other " number consisting of the like number of figures. Or, " any number, consisting of six or seven places of figures, " being proposed, he will determine, with equal expedition " and ease, *all* the *factors* of which it is composed. This " singular faculty consequently extends not only to the " *raising of powers*, but also to the extraction of the *square* " and *cube roots* of the number proposed; and likewise to " the means of determining whether it be a *prime* number " (or a number incapable of division by any other num- " ber); for which case there does not exist, at present, any " general rule amongst mathematicians." All these, and a variety of other questions connected therewith, are answered by this child with such *promptness* and *accuracy* (and in the midst of his juvenile pursuits) as to astonish every person who has visited him.

At a meeting of his friends which was held for the purpose of concerting the best method of promoting the views of the father, this child undertook, and completely succeeded in, raising the number 8 *progressively* up to the *sixteenth* power!!! and in naming the last result, viz. 281,474,976,710,656 he was right in every figure. He was then tried as to other numbers, consisting of one figure; all of which he raised (by actual multiplication and not by memory) as high as the *tenth* power, with so much facility and dispatch that the person appointed to take down the results, was obliged to enjoin him not to be so rapid! With respect to numbers consisting of two figures, he would raise some of them to the *sixth*, *seventh*, and *eighth* power; but not always with equal facility: for the larger the products became, the more difficult he found it to proceed. He was asked the *square root* of 106929, and before the number could be written down, he *immediately* answered 327. He was then required to name the *cube root* of 268,336,125, and with equal facility and promptness he replied 645. Various other questions of a similar nature,

nature, respecting the roots and powers of very high numbers, were proposed by several of the gentlemen present, to all of which he answered in a similar manner. One of the party requested him to name the *factors* which produced the number 247483, which he immediately did by mentioning the two numbers 941 and 263; which indeed are the only two numbers that will produce it. Another of them proposed 171395, and he named the following factors as the only ones that would produce it; viz. 5 × 34279, 7 × 24485, 59 × 2905, 83 × 2065, 35 × 4897, 295 × 581, and 413 × 415. He was then asked to give the factors of 36083; but he immediately replied that it had none; which in fact was the case, as 36083 is a prime number. Other numbers were indiscriminately proposed to him, and he always succeeded in giving the correct factors, except in the case of prime numbers, which he discovered almost as soon as proposed. One of the gentlemen asked him how many *minutes* there were in forty-eight years; and before the question could be written down he replied 25,228,800; and instantly added, that the number of *seconds* in the same period was 1,513,728,000. Various questions of the like kind were put to him; and to all of them he answered with nearly equal facility and promptitude; so as to astonish every one present, and to excite a desire that so extraordinary a faculty should (if possible) be rendered more extensive and useful.

It was the wish of the gentlemen present to obtain a knowledge of the method by which the child was enabled to answer, with so much facility and correctness, the questions thus put to him: but to all their inquiries upon this subject (and he was closely examined upon this point) he was *unable* to give them any information. He positively declared (and every observation that was made seemed to justify the assertion) that he did not know *how* the answers came into his mind. In the act of multiplying two numbers together, and in the raising of powers, it was evident (not only from the motion of his lips, but also from some singular facts which will be hereafter mentioned,) that some *operation* was going forward in his mind; yet that operation could not (from the readiness with which the answers were furnished) be at all allied to the usual mode of proceeding with such subjects: and moreover, he is entirely ignorant of the common rules of arithmetic, and cannot perform, upon paper, a simple sum in multiplication or division. But, in the extraction of roots and in mentioning the factors of high numbers it does not appear that any operation *can* take place; since he will give the answer *immediately*, or in a very few *seconds*, where it would require, according to the ordinary method of solution, a very difficult and laborious calculation: and moreover, the knowledge of a *prime* number cannot be obtained by any *known* rule.

It has been already observed, that it was evident, from some singular facts, that the child operated by certain rules known only to himself. This discovery was made in one or two instances, when he had been closely pressed upon that point. In one case he was asked to tell the *square* of 4395; he at first hesitated, fearful that he should not be able to answer it correctly: but when he applied himself to it he said it was 19,316,025. On being questioned as to the cause of his hesitation, he replied that he did not like to multiply four figures by four figures: but, said he, "I found out another way; I multiplied 293 by 293, and "then multiplied this product twice by the number 15, "which produced the same result." On another occasion, his highness the Duke of Gloucester asked him the product of 21,734 multiplied by 543: he immediately replied 11,801,562: but, upon some remark being made on the subject, the child said that he had, in his own mind, multiplied 65202 by 181. Now, although in the first instance it must be evident to every mathematician that 4395 is equal to 293×15, and consequently that $(4395)^2 = (293)^2 \times (15)^2$; and further that in the second case 543 is equal to 181×3, and consequently that $21734 \times (181 \times 3) = (21734 \times 3) \times 181$; yet, it is not the less remarkable that this combination should be *immediately* perceived by the child, and we cannot the less admire his ingenuity in thus seizing *instantly* the easiest method of solving the question proposed to him.

It must be evident, from what has here been stated, that the singular faculty which this child possesses is not *altogether* dependent upon his *memory*. In the *multiplication* of numbers and in the *raising of powers*, he is doubtless considerably assisted by that remarkable quality of the mind: and in this respect he might be considered as bearing some resemblance (if the difference of age did not prevent the justness of the comparison) to the celebrated Jedediah Buxton, and other persons of similar note. But, in the *extraction of the roots* of numbers, and in determining their *factors* (if any), it is clear, to all those who have witnessed the astonishing quickness and accuracy of this child, that the memory has *little or nothing to do* with the process. And in this particular point consists the remarkable

difference

difference between the present and all former instances of an apparently similar kind.

It has been recorded as an astonishing effort of memory that the celebrated Euler (who, in the science of analysis, might vie even with Newton himself,) could remember the first six powers of every number under 100. This, probably, must be taken with some restrictions: but, if true to the fullest extent, it is not more astonishing than the efforts of this child; with this additional circumstance in favour of the latter, that he is capable of verifying, in a very few seconds, every figure which he may have occasion for It has been further remarked by the biographer of that eminent mathematician, that "he perceived, almost "at a simple glance, the factors of which his formulæ "were composed; the particular system of factors belong- "ing to the question under consideration; the various "artifices by which that system may be simplified and "reduced; and the relation of the several factors to the "conditions of the hypothesis. His expertness in this "particular probably resulted, in a great measure, from the "ease with which he performed mathematical investiga- "tions *by head*. He had always accustomed himself to "that exercise; and, having practised it with assiduity, "(even before the loss of sight, which afterwards rendered "it a matter of necessity,) he is an instance to what an "astonishing degree it may be acquired, and how much "it improves the intellectual powers. No other discipline "is so effectual in strengthening the faculty of attention: "it gives a facility of apprehension, an accuracy and steadi- "ness to the conceptions; and (what is a still more valu- "able acquisition) it habituates the mind to arrangement "in its reasonings and reflections."

It is not intended to draw a comparison between the humble, though astonishing, efforts of this infant-prodigy and the gigantic powers of that illustrious character to whom a reference has just been made: yet we may be permitted to *hope* and *expect* that those wonderful talents, which are so conspicuous at this early age, may by a suitable education be considerably *improved* and *extended;* and that some *new* light will eventually be thrown upon those subjects, for the elucidation of which his mind appears to be peculiarly formed by nature, since he *enters into the world* with all those powers and faculties which are not even attainable by the most eminent *at a more advanced period of life*. Every mathematician must be aware of the important advantages which have sometimes been derived from

from the most simple and trifling circumstances; the full effect of which has not always been evident at first sight. To mention one singular instance of this kind. The very simple improvement of expressing the powers and roots of quantities by means of *indices*, introduced a new and general *arithmetic of exponents:* and this algorithm of powers led the way to the *invention of logarithms*, by means of which, all arithmetical computations are so much facilitated and abridged. Perhaps this child possesses a knowledge of some *more important* properties connected with this subject; and although he is incapable at present of giving any satisfactory account of the state of his mind, or of communicating to others the knowledge which it is so evident he *does* possess, yet there is every reason to believe that, when his mind is more cultivated and his ideas more expanded, he will be able not only to divulge the mode by which he at present operates, but also point out some *new sources* of information on this interesting subject.

The case is certainly one of great *novelty* and *importance:* and every literary character and every friend to science must be anxious to see the experiment fairly tried, as to the effect which a *suitable education* may produce on a mind constituted as his appears to be. With this view a number of gentlemen have taken the child under their patronage, and have formed themselves into a committee for the purpose of superintending his education. Application has been made to a gentleman of science, well known for his mathematical abilities, who has consented to take the child under his immediate tuition: the committee therefore propose to withdraw him, for the present, from *public exhibition*, in order that he may fully devote himself to his studies But whether they shall be able wholly to accomplish the object they have in view, will depend upon the assistance which they may receive from the public.

XXIV. *On the Existence of Caoutchouc in the Asclepias Vincetoxicum.* *By* J. Woodcock, *Esq.*

To Mr. Tilloch.

Sir, In addition to the notice contained in your last number of the discovery of caoutchouc in several vegetables growing in the vicinity of Philadelphia, by Dr. Barton, I beg leave to state that I have found this singular substance in in considerable proportion in the milky juice of the *Asclepias Vincetoxicum*.

Several summers ago, on breaking off a leaf from this plant, I remarked the exudation of a large drop of milky fluid; and being desirous of tasting it, with a view to ascertain whether it possessed the narcotic flavour of poppy, or lettuce, or the acrimony of *Euphorbium*, I found that it left in the mouth a tasteless and very elastic substance. On rubbing a drop of the milk also between the fingers, as the aqueous part became dissipated, the substance in question adhered strongly to them, and was capable of being drawn out into long and very elastic threads. I collected about two drachms of this juice in a phial. On immersing the bottle containing it in hot water, the milk had its fluidity increased, but no separation of its parts took place. When dropped into distilled water, it sunk to the bottom; and if diffused by agitation, it separated by repose into white flocculi: this separation is immediately effected by heating the mixture. An instantaneous coagulum is formed on dropping the juice into deaquated alcohol, and the substance so separated is readily soluble in naphtha. It is also very inflammable. These are all the observations that I have yet had opportunity of making. If they have not already been noticed, I shall be obliged by an early insertion of them in your useful work.

I am not aware whether this principle is common to all the *Asclepiadeæ*, having only tried the *vincetoxicum* and *fruticosa*.

I am, sir,
Your very obedient servant,
J. Woodcock.

River Terrace, Islington,
August 6, 1812.

XXV. *A Memoir upon the Organs of Absorption in Mammiferous Animals. By M.* Magendie, *Doctor of Medicine of the Faculty of Paris, Professor of Physiology, &c. The Experiments conducted by Doctors* Magendie *and* Delile*.

Among the facts which I had the honour to report to the Class, in a memoir upon the *upas ticuté*, the *nux vomica*, and the bean of St. Ignatius, there is one which appeared to me worthy of more particular attention; I mean the readiness with which those poisonous matters are absorbed

* Read at the Institute on the 7th of August, 1809.

and

and introduced into the sanguiferous system. It must be recollected, that it scarcely requires twenty seconds to convey these poisons from the peritoneal cavity to the spinal marrow.

The general received ideas relative to the organs of absorption do not admit of a doubt that the lymphatic vessels are the agents for conveying these poisons into the sanguiferous system. Thus, in the experiment where the poison was introduced into the middle of the thigh of an animal, there was but one way of explaining its absorption: it must necessarily be admitted that it was taken from the wound by the lymphatic vessels of the parts with which it was in contact, that after being absorbed, it was carried by these vessels towards the glands of the groin; that after traversing these bodies, it was conveyed, still by the lymphatic vessels, to the thoracic duct; finally, that it was introduced into the sanguiferous system by the communications which the thoracic duct preserves with the subclavian veins, and principally with those of the left side.

Such ought to have been, and such in fact was our opinion at the time of the publication of the memoir upon the strychnos. Nor were the experiments of which I am about to give an account undertaken with a view to discover new facts, but rather to add a degree of certainty to an explanation already admitted; and our labours did not take a particular direction, until a great number of facts obliged us to modify our view of this subject. But so rapid an absorption, by vessels whose principal characteristics are weakness and slowness of action; a poisonous substance, that so quickly pervades the difficult and winding route of the lymphatic glands without any alteration therein, were two circumstances that ought, perhaps, to have made us entertain some doubts of the correctness of the received explanation. This explanation, however, is given by so many respectable persons, and is supported by experiments so positive, that even now, when we have many facts to oppose it, we dare not say that it wants exactness, but only that it is not admissible under every circumstance.

Previous to any detail of our experiments, it will not be useless to relate, in few words, an opinion which for some time balanced the present prevailing belief relative to the organs of absorption.

This opinion, professed by Boerhaave, Haller, Méckel, Ruysch, Swammerdam, and others, was, that the sanguiferous

ferous veins, in common with the lymphatic vessels, possessed an absorbing power.

It is supported by different circumstances of structure, and by some physiological and pathological facts. A series of interesting experiments, undertaken and executed a few years since, at the veterinary school of Alfort, has also strengthened the probability of such absorbent property of the veins, but without producing entire conviction. It is well understood, that an opinion established upon the physical structure of the organs, deduced from a sufficient and conclusive number of experiments, and supported by the names of Boerhaave, Haller, and Ruysch, ought not to be easily abandoned. Nor was less required than the anatomical discoveries of the last century, the correct experiments of Hunter and his brother, those of Cruikshank, Mascagni, Desgenettes, and others, to establish the belief that the lymphatic vessels only possess the absorbent faculty.

I will further cite, in support of the general opinion, some very curious experiments lately made by M. Dupuytren. This physiologist, who has kindly permitted me to report the principal results of his labours, tied the thoracic duct in several horses; some of them died in five or six days, others preserved every appearance of health. We know already, by an experiment of Duverny, by some observations on the thoracic duct when obstructed, and above all by the experiments of Landrin, that the thoracic duct may cease to convey the chyle into the subclavian vein, without being followed by the death of the animal: it is true, we also know, that some animals died in consequence of a ligature round the duct; but we are entirely ignorant of the cause of this diversity in the results. M. Dupuytren, by his experiments, has found one very satisfactory. In the animals that died in five or six days from the ligature round the thoracic duct, he always found it impossible to pass any injection from the inferior part of the duct into the subclavian vein; consequently, it is very probable that the chyle ceased to be conveyed into the venous system immediately after the application of the ligature. On the contrary, in all the animals who have survived the application of the ligature, it has always been easy to make every kind of liquid pass from the inferior part of the duct to the subclavian veins, by means of the very numerous communications between these two points by the lymphatic vessels, placed alike in the posterior as in the anterior mediastinum.

I have

I have personally assisted M. Dupuytren in opening a horse, the thoracic duct of which he had tied more than six weeks before, and I easily satisfied myself that there existed evident communications between the inferior portion of the duct and the subclavian veins, although this canal was entirely destroyed at the place of the ligature.

I now proceed to the experiments which I made, for the most part in concert with M. Delile, to determine whether the lymphatic vessels are really the only ones by which foreign substances enter into the venous system.

A circumstance that has always thrown some obscurity over the experiments upon absorption, is the difficulty of demonstrating, with certainty, the passage and presence of absorbed matters, either in the lymphatic or sanguiferous vessels. We have not these inconveniences to fear in employing the *upas*, or the *nux vomica;* for it is known that two centigrammes of these substances produce effects too remarkable to be mistaken.

Would the ligature round the thoracic duct stop the passage of the poison in the sanguiferous system, and consequently its effects upon the spinal marrow? This was the first question that we proposed to resolve.

I applied in a dog a ligature to the thoracic duct, a little before its opening into the left subclavian vein; I afterwards introduced a solution of the *upas* into the cavity of the peritoneum. The effects of the poison were as prompt and marked as if the thoracic canal had not been tied. I have tried the same ligature in other animals; but instead of introducing the poison into the cavity of the peritoneum, I introduced it either into the pleura, or into the stomach, the intestines, or muscles of the thighs, &c. The effects have always been equally rapid and intense, as if the thoracic canal had been free.

Decisive conclusions cannot be drawn from these first essays, for we know that the thoracic canal is not the only point of communication between the lymphatic and venous systems. There is ordinarily, on the right side, a second thoracic duct, almost as considerable as that on the left; the large lymphatic vessels often open themselves solitarily into the subclavian veins; and more frequently still the thoracic duct has several mouths into the veins where it terminates.

One of these circumstances was found to occur in the animals subjected to our experiments; and we must have recourse to other trials, from which other results, less equivocal, may be deduced.

Upon a dog who had eaten a large quantity of meat seven hours before, in order that his abdominal lymphatic vessels might be easily perceived, we made an incision through the parietes of the abdomen, and took from thence a coil of the small intestines, upon which we applied two ligatures about five inches apart from each other. The lymphatics, which had their origin in this turn of the intestine, were very white and very apparent from the chyle which they contained. Two ligatures were placed at the distance of one centimetre upon each of these lymphatics; we cut these vessels between the two ligatures; we took great care in our experiment, and assured ourselves, by every possible means, that the curvature of the intestine, taken from the abdomen, had no further communication with the body by the lymphatic vessels. Five arteries and five mesenteric veins ended in the portion of the intestine comprised between the two ligatures; four of these arteries and four of the veins were tied and cut in the same manner as the lymphatics; the two extremities of the curvature of the intestine were cut, and entirely separated from the rest of the small intestine. Thus we had a portion of the intestine about five inches in length, not communicating with the rest of the body, but by one artery and one mesenteric vein; these two vessels were separated at the distance of four fingers breadth. We even raised the cellular substance, lest some lymphatic vessels might lie there concealed. We had nothing further by which to obtain a positive result, than to inject a small quantity of the *upas tieuté* into the cavity of the intestinal curvature. This was also done with proper precautions to prevent the escape of the injected liquor. The curvature of the intestine, enveloped in fine linen, was replaced in the abdomen; it was then precisely one o'clock. To our great astonishment, about six minutes after, the general effects of the poison diffused themselves with their usual intensity, and in such a manner that every thing proceeded as if the curvature of the intestine had been in its natural state.

The animal being dead, we examined the parts; no ligature was displaced, nothing could make us suppose that the poison had passed into the abdominal cavity.

This experiment, repeated several times without any modification in the result, appeared to us most conclusive; it proves, at least as far as can be proved in physiology, that the lacteal vessels are not the exclusive organs of intestinal absorption.

This kind of absorption, different from that of the lymphatic, may be peculiar to the intestines; it would be important to know, if it could be discovered to exist in the other parts.

We

We separated from its body the thigh of a dog, previously rendered comatose by opium, (in order to spare it the pain of a difficult experiment;) this separation was so made, that the thigh still communicated with the trunk by the *crural* artery and vein. We took, with regard to these two vessels, the same precautions as for the mesenteric vein and artery in the preceding experiments; that is, we isolated them on an extent of four centimetres, and raised their cellular *coat*, lest it should conceal some lymphatic vessels; we then inserted two grains of the poison in the foot, and waited the effects. They manifested themselves with as much promptitude and energy as if the thigh had not been separated from the body; insomuch that the first signs of the action of the *upas* appeared before the fourth minute, and the animal died before the tenth.

It may be objected, that notwithstanding all the precautions taken, the arterial and venous parietes still contained some lymphatics, and that these vessels were sufficient to give passage to the poison. It were easy to refute this objection.

I repeated, upon another dog, the preceding experiments, with this modification, that I introduced into the crural artery a small quill, upon which I fixed this vessel by two ligatures; the artery was afterwards cut circularly between these two bands. I did the same to the crural vein; so that there was no longer any communication between the thigh and the rest of the body, unless by the arterial blood which comes to the thigh, and by the venous blood which returns to the trunk. The poison introduced into the foot produced its general effects in the ordinary time, that is, in about four minutes. It may be deduced, I believe, from these different experiments, that the lymphatic system is not, at least in certain cases, the exclusive route that foreign substances take to arrive at the venous system.

This new mode of absorption, much more direct than that by the lymphatics, presents the means of easily conceiving the rapidity with which the different deleterious and other matters are absorbed, as well as the rapidity with which they produce their effects upon the system.

But what are the organs that first absorb the poison from the parts where it has been introduced? Are they the radicles of the veins, or are they rather the capillary lymphatics, which, having immediate anastomosis with the sanguiferous capillaries or exhalants, would immediately diffuse the poison through the venous system?

The experiments which I have just related, joined to those

those which have been made on the same subject, appear to me totally insufficient to decide either of these questions; only it ought to be remarked, that our experiments are strongly in favour of a direct absorption by the veins.

But it is a fact rendered evident by the preceding experiments, and upon which it is necessary to pause a moment, that the venous blood becomes charged with the poison; and that, by the intervention or means of this blood, the poison produces its deleterious action upon the system. In fact, if in the experiments where I had separated the thigh from the trunk, we suspend the course of the venous blood, by compressing between two fingers the crural vein, we lessen, and even totally suspend, the production of the effects. The blood of an animal, in which the signs of action of the *upas* are developed, contains then some portion of poisonous matter; indeed it may be said to be really poisoned. It were curious and interesting to know, if this blood, carried into the circulatory system of a healthy animal, would produce effects similar to those it had upon the animal itself. At first sight, we are led to believe that this is extremely probable, even that it is certain. The following experiments will show with what care we ought, in physiology, to distinguish that which is probable from that which is proved by experiment.

We passed the arterial blood of an animal, in which the tetanus caused by the *upas* was manifest, into the jugular vein of a healthy animal; the transfusion lasted near twenty minutes, so that the healthy animal received a very considerable quantity of poisoned blood, which at the first moment of the experiment was of a red and vermilion colour, and which afterwards became violet and black, when the *upas* had produced asphyxia. There did not, however, appear any irritation of the spinal marrow, and the animal only experienced what happens in common transfusions made with care. I mean, that it had for some hours a very marked acceleration of the inspiratory and expiratory motions, as well as a very abundant pulmonary exhalation. Frequently repeated, this experiment has always produced the same results.

We were now certain, that the arterial blood of animals poisoned by the *upas tieuté*, the *nux vomica*, or the *bean of St. Ignatius,* was not susceptible of producing similar effects on other animals; it would not be, perhaps, the same with the venous blood. It may be presumed, that the respiratory action changed the nature of the poisonous substance; and this alteration might, to a certain degree, give the

the reason, why the transfusion of the arterial blood of animals poisoned by the *strychnos* is not followed with bad effects.

This circumstance did not take place with the venous blood, which returns from the part where the poison has been introduced. After the experiments reported in the memoir upon the *upas*, and in this, it is impossible to doubt, that this blood does not transport the poison to the lungs. It is very probable that, introduced in the circulatory system of another animal, it would produce effects similar to those which it caused upon the animal on which the inoculation of the poison was made.

A small piece of wood covered with two grains of *upas tieuté* was stuck into the thick part of the left side of the nose of a dog. Three minutes after this introduction, we passed into the venous system of another dog, the blood of the jugular vein of the side where the introduction of the poison had been made. The transfusion commenced about one minute before the first signs of the *upas*; it did not cease until the death of the animal who experienced it. No appearance of irritation of the spinal marrow was perceived in the animal who received so great a quantity of poisoned blood.

Although these experiments were repeated several times, with variations in the mode of introducing the poison, we never could perceive in the healthy animal, who had suffered the transfusions of poisoned blood, any thing which resembled the effects of the *strychnos*.

Results so positive, appear to us of a nature to warrant the conclusion, that the venous blood of animals poisoned by the *upas*, the *nux vomica*, and the *bean of St. Ignatius*, is no more capable than the arterial blood, of producing upon another animal the effects which it will cause upon the animal from which it was taken.

If there still remained any doubts, they would be removed by the following experiment, which was repeated several times.

As in the experiments above related, we separated, from the body, the thigh of an animal, isolating as before the crural artery and vein; we introduced the poison into the separated foot, and transfused the blood of the crural vein into the jugular vein of a sound animal. The passage of the blood from one animal to the other lasted more than ten minutes, a time more than sufficient for the production of the effects of the *upas*. But no sign of the action of this poison was perceived, either in the one or in the other

 animal,

animal. The one preserved perfect health; the other died in a few days, in consequence of the amputation of the thigh, and the loss of blood which was transfused.

It must not, however, be thought, that in this experiment the transfused blood, by some particular cause, had no deleterious properties, for the following experiment proves the contrary.

As in the preceding experiment, I separated the thigh from the body; three minutes after introducing the poison into the foot, I passed the blood of the crural vein into the jugular vein of another animal; the transfusion was prolonged five minutes without producing any effects. I then stopped it, and disposed things in such a manner that the blood of the crural vein should return to the animal to which it belonged. Almost instantly this animal exhibited evident signs of the action of *strychnos* on the spinal marrow.

From the different experiments reported in this memoir, we must, I think, conclude,

1st. That the lymphatic vessels are not always the route followed by foreign matters, to arrive at the sanguiferous system.

2d. That the blood of animals, upon which the bitter *strychnos* has produced its deleterious effect, cannot produce any fatal effects upon other animals.

As to the explanation of this singular phænomenon, it would be, I think, premature to give it at present. In physiological science, we ought to be sparing of conjectures, and prodigal of facts.

Magendie, D. M. P.

Note.—This Memoir has received the approbation of the Institute.

XXVI. *On the Production of Sugar from the Starch of Wheat, and of Potatoes, by the Agency of Sulphuric Acid. By Mr.* William Moore, *London.*

The interesting nature of this curious fact will, I hope, be thought sufficient apology for my venturing to offer a few observations on this subject. I shall first briefly state that it is to M. Kirchhoff of St. Petersburg that the public are indebted for this discovery. He very lately communicated it to Professor Berzelius of Stockholm. This celebrated chemist, immediately on his arrival in this country, mentioned it to Sir Humphry Davy, at whose desire I repeated the

the experiments, and by whose kindness I have been allowed to operate in the Laboratory of the Royal Institution.

The original experiment was made by boiling together 100 parts of starch, 400 parts of water, and one part of sulphuric acid, for 36 hours, on a slow fire, in a covered vessel, adding from time to time water for that which boiled away: towards the expiration of that time some charcoal in powder was put in; and lastly, enough of carbonate of lime to saturate the acid: the solution was then filtered, evaporated to the consistence of a thick syrup, and put to crystallize; which generally takes place in three days. In repeating this experiment I got some starch not converted, which was owing I believe to the heat not being sufficiently strong, with the intention of shortening the time of boiling. I increased the quantity of sulphuric acid (at the suggestion of Professor Berzelius in a second trial) to 3 per cent. I have also made trials with 4, 5, 6, and 8 per cent. of acid, but they did not either increase the result, or shorten the time of boiling, as much as I expected. I have also made experiments with the starch of potatoes, in the same proportions as that of wheat which I used in my first trials: the only difference I could perceive between them was, that the sugar was not so abundant from the potatoe starch, and that it contained more gummy matter than the sugar obtained from that of wheat. I observed that they do not possess the property of sweetening liquids, at all equal to the sugar from the juice of the cane. I have not been able to procure either of them sufficiently crystallized, to state the difference of either of them (if any) from each other, or from any other sugar. My experiments at present are not sufficiently complete to warrant my giving either a statement of the quantity produced, or of the expense of the process; indeed this would be foreign to my first object, which was to repeat the experiment; but I am convinced it cannot be an object at present in Britain. I am assured it was lately in contemplation to adopt it on a large scale in the Russian empire, as the price of cane sugar was increased so much, and the supply was so uncertain.

I shall not presume to give the rationale of this process at present. I am ignorant of that advanced by the able discoverer (M. Kirchhoff); but I must dissent from the opinion given by M. Nasse*, who thinks the sulphuric acid is decomposed. I have weighed the quantity of lime it required to saturate the acid used in the experiments; and also of

* See *Journal de Physique* Mars 1812, page 199, &c. which did not arrive in this country until after these experiments were made.

that used to saturate an equal weight of the same acid diluted with water, and I find them to agree exactly.

M. Vogel *, in his remarks on this subject, seems to think that increasing the quantity of acid hastened the formation of the sugar. He only obtained the syrup; time did not permit him to go further: we are therefore, for the present, deprived of that able chemist's remarks on this subject. Should my future experiments on this subject prove sufficiently interesting, I shall have great pleasure in making them public, and I will take the liberty of asking indulgence for these, as I am but a pupil in that science in which there are at present so many able proficients. If these remarks should induce some able investigator to pursue the subject, and thereby benefit science and the arts, I shall consider myself as very fortunate.

XXVII. *On the Barometer. By* Richard Walker, *Esq. Oxford. Communicated by the Author.*

To Mr. Tilloch.

Sir, Should the following observations, forming the conclusion of a subject, viz. "Observations on the Barometer," which you did me the favour to communicate, through the channel of the Philosophical Magazine for October 1810, and the two following numbers, be deemed not unworthy of a place in your next number, I request you will have the goodness to insert them.

I am, sir, your obedient servant,

Oxford, August 17, 1812. Richard Walker.

P. S.—I have judged it expedient, in the course of these observations, to descend occasionally to particulars which, to the generality of your readers, must be totally unnecessary, but which to some persons, who may not before have given much attention to this subject, may not prove unacceptable.

The reason of the long interval between this and my former communications on the same subject, is in consequence of my attention having been engaged in a small work of a different nature, which I hope soon to have it in my power to announce.

The barometer, *stationary*, with *a concave surface* of the mercury at the top, is an almost infallible indication of

* *Journal de Physique* Mars 1812, Notice sur la transmutation de fecula de froment en matière sucrée, par M. Vogel.

rain,

rain, at the place of observation, or in its vicinity; especially if the barometer be *at* or *below* CHANGEABLE.

The barometer, *stationary*, with a *convex surface* of the mercury at the top, is a strong indication of *fair weather*, at the place of observation, or in its vicinity; especially if the barometer be *at* or *above* CHANGEABLE.

In the first instance, I would account for this circumstance, by the natural spring or elasticity of the air being *suspended* or *diminished* (the density or weight of the atmosphere remaining the same) by the *intervention of vapour* collecting into a mass; and in the latter instance, from the *dispersion* of the interposed vapour, by which the natural spring or elasticity of the air is restored.

For the same reason it is, I presume, that during a steady fall of *rain*, the barometer is commonly *stationary*, with a *concave surface* at the top; and as soon as the weather begins to clear up, the top of the mercury in the barometer assumes a *convex surface*.

The quick *ascent* into the atmosphere, of *smoke, vapour*, &c. indicates *fair-weather*; and the slow ascent, and particularly the *descent* of them, indicates *rain*. The former, in consequence of an influx of *denser* or *heavier* air, into the lower stratum of the atmosphere, commonly from the *north* and *east* points; and the latter, in consequence of an influx of *rarer* or *lighter* air, into the lower stratum of the atmosphere, commonly from the *south* and *west* points.

Upon the whole there is more *wet weather*, whilst the barometer is *above* CHANGEABLE; than there is *dry weather*, whilst the barometer is *below* CHANGEABLE. Hence it follows that, *cæteris paribus*, the former state of the barometer is not so strong an indication of *fair weather*, as the latter is of *rainy weather*.

The barometer *rising*, especially if it be progressive, whilst the wind is in the *south* or *south-west* points, indicates a change of the wind to the *north* or *east points*; and conversely, the barometer *sinking*, whilst the wind is in the *opposite points*, indicates a change to the *south* or *west points*.

In the *spring*, and as *summer* advances, *rain* brings successively *warmer weather* after it; and in *autumn*, and as *winter* approaches, *rain* brings successively *colder weather* after it. Hence, successive showers gradually change, as it were, *spring* into *summer*, and *autumn* into *winter*.

The reason of this seems to be, that during *fair weather alone*, the true character of each season, with respect to temperature,

temperature, is exhibited; whereas it is in some degree obscured, or suspended, during *cloudy weather*.

The mercury in the barometer is usually *fixed*, at whatever height it may happen to be, with a *concave surface*, during *settled rain;* and *fixed* with a *convex surface*, during *settled fair weather*. It is essential, in order to make accurate observations on the barometer, that it be viewed with a magnifier, which is best when *fixed* to the moveable index.

Fogs in the *morning* are more visible over *land* than over *water*, because the *greater coldness* of the land *condenses the vapour most;* but in the evening, over the *water*, because *water* is then the *coldest*.

The *mean height* of the clouds above the earth's surface is about three quarters of a mile;—the *greatest height*, somewhat more than a mile—and the *least height*, about three furlongs, and sometimes in *thunder storms*, even less.

In order to make the most accurate observations on the state of the weather, and to predict the various changes which are about to take place in it, the observer should be provided with a *barometer*, *thermometer*, and *hygrometer*, each of the best construction. De Luc's hygrometer, I think, is the best;—and in order, at all times, to be able to notice the changes in the direction of the wind, which are taking place, and which is extremely essential, it would be expedient that a wind-vane be so constructed, as to exhibit the direction of the wind within the room appropriated for meteorological observations; which, in defect of any better means, might be contrived, by the axis of the *wind-vane*, without being continued so as to reach within the room, having an *index* affixed to it.

The *rising* of the barometer is a more certain indication of *fair weather*, than its *sinking* is of *rainy weather;* because it sinks for wind, as well as rain. If therefore, whilst the barometer is sinking, the atmosphere still remain clear, *wind* may be expected.

In *winter*, *spring*, and *autumn*, a sudden falling of the mercury presages *high winds* and *storms*; but in *summer*, *heavy showers*, and sometimes *thunder*.

The principal changes in the atmosphere take place about the time of the *vernal equinox*, viz. from the 16th to the 28th of *March*.

Rain or *snow* may come during a *north* and *easterly* wind, whilst the barometer stands higher, than when the wind is in the *south* or *west* points; because in the former instance

instance the clouds come from a *denser* to a *rarer medium*, and in the latter case, from a *rarer* into a *denser medium*.

The *maximum* and *minimum* heights of the barometer, in this and every other country, are limited, by nature, thus: When the atmosphere is charged with as much *water* in a state of *chemical combination*, under the most favourable circumstances for absorbing it, as the atmosphere can sustain, it is then *heaviest*, and the barometer is at its *maximum*;—and when the air is most free from, or contains the least quantity of, *water* in a state of *chemical combination*, other circumstances concurring, the atmosphere is then *lightest*, and the barometer is at its *minimum* of height. Hence, *cæteris paribus*, the absorption of water, by air, renders the atmosphere *heavier*, and its parting with it renders it *lighter**.

During many successive years of observation on the barometer in England, it may be found, that its *greatest height* will not exceed one inch and a quarter *above* CHANGEABLE; nor its *least height*, the same distance *below* CHANGEABLE.

The variations in the barometer are *greatest* towards the *poles*; less, in *temperate latitudes*; and *least*, between the *tropics*.

The barometer was observed, some years ago, at Oxford, as low as 28 inches, within two hundredths, at which time the weather was perfectly fair and calm; but it appeared afterwards that an earthquake had happened at the time, at a distant part of the world. This circumstance puzzled ordinary observers, and injured the reputation of the *weather-glass*; but the intelligence afterwards of the event which had happened restored its character.

The *moisture* on the ground and in other situations *freezes*, as is well known, when the atmosphere is *warmer* than 32°, the freezing point, in consequence of the cold produced by *evaporation*. Under the most favourable circumstances, viz. a *dry air* and a *brisk wind*, this may happen, when the air is eight or ten degrees above the *freezing point*. The *rise* or *fall* of one-tenth of an inch, in the barometer, is usually accompanied, or followed, by a very perceptible change in the weather.

* Among other circumstances, which show the power of air for absorbing water or vapour, the following is a very curious as well as satisfactory one: if a small cloud, or fragment of a cloud, in a loose or rare state, insulated from others, in a clear part of the atmosphere, especially when the air is warm and calm, be observed, it will be found to melt, or dissolve, as it were, in the atmosphere, gradually, and at length to vanish entirely.

Rain happens, among other causes before mentioned, in consequence of a subtraction of the *electric fluid*, which, when the air abounds with vapour, is easily conducted to the earth; hence arises the *velocity* of rain in *thunder showers*.

In this climate, the *hottest* time of the day is about three in the afternoon, in consequence of the *continued influence* of the sun, whilst near *its greatest height* from the horizon; and the *coldest* is *a short time before sun-rise*, viz. that time at which our part of the earth has been *longest* without the influence of the sun.

The *hottest month* is *July;* and the *coldest month* is *January;* the former, in consequence of the continued strongest influence of the sun, whilst *near its greatest elevation* from the horizon; and the latter, in consequence of the *continued weakest influence* of the sun, whilst near its *least elevation* from the horizon. The *driest months* are *July* and *August*, and the *wettest* are *October* and *November;* the former, in consequence of the small quantity of water which has been raised into the atmosphere previously, and the powerful influence of the sun, during these months, in dissipating the vapours which are raised; and the latter, in consequence of the *inverse effects* of those two causes.

In like manner, as the *annual* influence of the sun operates upon the weather, producing different seasons, respecting wet and dry weather, before stated; it is not improbable, that its *diurnal influence*, being a kind of *epitome* of the former, may, in a smaller degree, have the same effect in producing an *effort* for fair weather, whilst the sun is nearest its *greatest diurnal altitude*, in consequence of its greater power, at that time, of dissipating vapours, or dispersing clouds.

The greater or less avidity with which the mercury appears to *rise* or *sink*, as exhibited by the greater or less *convexity* or *concavity* of the mercury at the time of observation, the stronger or weaker will be the indication in each instance.

The surface of the mercury in a barometer is best seen when the tube, at the part to be observed, is exposed to the light behind, that is, constructed without an opake substance at the back of it.

The *convex* or *concave* surface of the mercury arises, of course, from the adhesion of the mercury to the tube, in *rising* or *sinking:* at each observation of the barometer, therefore, after noticing the variation in height, the *index* should

should be first accurately placed, the barometer then smartly rapped, in order to reduce the mercury to a level, and the *index* again accurately adjusted.

The principle of a barometer is so perfectly uniform in its effect, that although, for want of precision in their construction, any two of them may not, under similar circumstances, respecting the state of the atmosphere, and the elevation in which they are placed, stand precisely at the same height; yet all will agree in this—they will rise or sink at the same time.

The ordinary upright barometer is the only one in use, among scientific persons, of which there are two kinds, viz. the cistern or bason barometer, and the syphon barometer: the former of these requires a *floating gage*, with an adjusting screw, in order to show the precise length or height of the column of mercury in the tube; which is essential in accurate observations, and particularly in the measurement of altitudes by the barometer; but which may be dispensed with in ordinary observations.

The only objectionable circumstance in the *perpendicular barometer* is the shortness of its scale; but, since there are no means of extending the scale of a barometer without, in some degree, injuring its principle, this defect is supplied by a *nonius* or *vernier*.

Barometers intended for the purpose of *measuring altitudes*, are fitted up in a peculiar manner adapted to that intention.

The attractive power of the *sun* upon the earth's surface, is to that of the *moon*, as 2 to 5.

With respect to the direction of the wind, it is tolerably uniform within the *tropics*, being more immediately under the influence of the two principal causes of currents of air, viz. first the *sun*, which, in its course heating the places it successively passes over, causes a very considerable rarefaction of the air there, which is constantly followed by a current of air, to restore the equilibrium;—and secondly, the current of wind, which is produced by the *attractive* influence of the *sun* and *moon* on the atmosphere, as the different places pass under them.

Near the *poles*, the wind is probably more uniform than it is in the middle or temperate climates; the lower portions of the air being constantly inclined towards the *equator*, in consequence of the *increasing heat* in that direction; and the upper portions being renewed, from the more elevated regions of the *inter-tropical* currents.

Whereas, in the middle or *temperate* climates, besides other causes unknown to us, the direction of the wind becomes

comes more complicated, in consequence of its situation between the two former; being occasionally influenced by each of them, separately or conjointly.

There seems to be a kind of habit in the weather, as in other cases: thus, towards the close of a series of *dry weather*, several efforts are apparently made towards a series of *rainy weather*, before it actually sets in; and *inversely*.

With respect to the difference of the weather, which I have presumed *takes* place during the *increase* and *wane* of the moon; I would account for it on a supposition that at each conjunction of the moon with the sun, or the time of the *new moon*, as it is called, there may be a kind of disposition, as it were, for *clarifying* the atmosphere, or *renovation* of fair weather; and *vice versâ*, at the moon's opposition, or *full moon;* and the reason why this change does not actually take place until about the third or fourth day after each change of the moon, because, as I have stated before, the former habit of the weather is not till then entirely conquered.

The atmosphere becoming *drier*, as indicated by the *hygrometer*, indicates *a disposition* for *fair weather*, and *vice versâ*.

A cold, dry, and serene state of the atmosphere is most favourable for barometrical measurements.

The limit, or boundary of the atmosphere, terminates where the expansive force or power of the air, *upwards*, or *from* the earth's surface, is completely overcome by the force or power of gravitation *downwards*, or *towards* the earth's surface, which is estimated at about 44 or 45 miles*.

A pint measure of air, when the barometer is at $29\frac{1}{2}$ in ENGLAND, weighs eight grains, or a gallon 64 grains; but near the POLES somewhat more; and at THE EQUATOR somewhat less. This variation in the weight of the air, in the different parts of the world, is ascribed to a difference in the height of the atmosphere; by which it happens, that the air near the surface of the earth, at different places, differs in *density;* the pressure or weight of the incumbent atmosphere being the same. The atmosphere is presumed to extend *highest* at the equator, and to be *lowest* at the poles; in consequence of a greater degree of *centrifugal force* and *heat* at the equator.

The barometer, *cæteris paribus*, stands *higher* in winter than in summer, in consequence of the greater condensation of the air, from cold, at that season.

* The atmosphere, it is computed, would extend only to about $5\frac{1}{2}$ miles in height, if it were so compressed, that its density throughout should equal its degree of density near the surface of the earth.

The

The barometer unquestionably has been observed, in England, to rise *above* 31, and to sink *below* 28; viz. to exceed those limits, each way, by the *tenth* of an inch.

The lowest point at which I have observed the barometer is $28\frac{1}{4}$ inches, viz. on November 9th, 1810; which was accompanied by a remarkably high wind, and torrent of rain.

The ordinary range, or variation, in the barometer, differs according to the latitude of the place:—thus, at the *equator* (except during the hurricane season) it is scarcely any thing;—and at the *poles*, it is probably not less than four inches; varying in an *increasing ratio*, from the *equator*, to each pole*.

The column of mercury in the tube of a barometer, as most persons know, is sustained by the counter-pressure of a column of air, of the same base, extending to the top of the atmosphere; which, when at its mean state of gravity, that is, when the barometer is at $29\frac{1}{2}$ inches, presses on the base of the mercury, with a weight which is equal to, or after the rate of, fifteen pounds upon each square inch of surface.

In a comparison of barometers placed in different situations, with respect to elevation; an allowance must be made, at a mean rate, of 100dth of an inch on the barometer for each complement of nine feet measure in height; that quantity being *added* to the higher barometer, or *subtracted* from the lower one, in order to make the comparison equal †. (See *Table* I. *&c. page* 279, *of Philosophical Magazine, for October* 1810.)

It has been confidently asserted by respectable authors, that the barometer is affected by the *atmospherical tides;* it does not appear, however, that any observations have actually proved the fact.

With respect to the periodical *rise* and *fall* of the barometer noticed at *Calcutta;* in the first place, the variation appears to have been very small; and *secondly*, it does not correspond in point of time with the *attractive influence* either of the sun or moon.

That the atmosphere is influenced by the *attractive power* of the sun and moon, there can be no question; but it seems

* The barometer at PERU, under the EQUATOR, varies ordinarily, only to the extent of two or three-tenths of an inch; but at PETERSBURGH, to the extent of three inches.

† The difference in the height of two barometers equally well constructed, one being in the ground floor, and the other in the attic, will of course amount to *several hundredths* of an inch.

contrary

contrary to reason, that the barometer should be affected by it; since, in proportion as the atmosphere is *rarefied*, from this cause; in the same proportion, from the same cause, likewise, is the atmosphere *accumulated* there; and hence the *gravity*, or *pressure* of the atmosphere upon the surface of the earth, remains the same in each instance.

I might add, that if an alteration in the barometer actually took place from such a cause, it could scarcely have escaped my notice.

P.S. I have noticed with considerable sensations of surprise and satisfaction, the progress of Professor Leslie, in his experiments *on the production of cold by means of the air-pump*, in which it is stated that that gentleman has already reached a cold of 70° below 0. At the time I was pursuing my experiments on *artificial cold* by frigorific mixtures, the means of producing *artificial cold* by means of the air-pump naturally presented itself, and to which I was more particularly stimulated by an experiment related by Mr. *Nairne*, in which it is said, by the evaporation of ether, under the air-pump, a cold of 48° *below* 0 was produced, the temperature of the air in the room being 55°. *Philosophical Transactions* 1777.

I made several attempts at producing an extraordinary degree of cold by this means, but without success; owing chiefly, as I presumed, to the imperfection of the best air-pump I was able to procure. Conceiving that the *Torricellian vacuum*, as it is called, might be the best method of effecting the most sudden and complete evaporation of ether, upon which the success of the experiment depends, I had projected in my mind to put this method to the test; but the difficulty of obtaining a fit apparatus for the purpose prevented me.

Perceiving that Professor Leslie's experiments, in which he has succeeded to a most extraordinary degree, have been conducted upon the principle alone of *rarefying the air*, without any auxiliary means excepting his ingenious method of absorbing the vapour, the existence of which necessarily diminishes the success of the experiment; I have been induced to take this opportunity of mentioning these circumstances, fully trusting that, should these observations furnish no hint to the learned and ingenious Professor, they will not be deemed an improper interference *.

* By means of evaporation in the open air, from purified *sulphuric ether*, (as mentioned in my *Treatise on the Production of Cold*, pages 80, 81,) I sunk the thermometer from 71°, the temperature of the air at the time, to 12°; and by *water* used in a similar manner, to 56°.

XXVIII. *No-*

XXVIII. *Notices respecting New Books.*

Elements of Chemical Philosophy. By Sir Humphry Davy, LL.D. Sec. R S. Prof. Chem. R.I. and B.A. M.R.I. F.R.S.E. M.R.I.A. Member of the Royal Academy of Stockholm; of the Imperial Med. and Chir. Academy of St. Petersburgh; of the American Philosophical Society; and Honorary Member of the Societies of Dublin, Manchester, the Physical Society of Edinburgh, and the Medical Society of London. Part I. Vol. I. pp. 527, 8vo. 1812. Johnson and Co., St. Paul's Churchyard.

Great as was the expectation of this work, its importance will defeat disappointment. It abounds in luminous views, original conceptions, comprehensive principles, and familiar yet sublime, grand yet simple, developments of the laws and œconomy of the inanimate world. The impressive simplicity of the general principles can only be equalled by the endless variety of curious and important facts brought forward to elucidate or familiarize every position. But an abstract of the author's plan of arrangement, and some extracts from his original principles, will best convey an idea of the nature of these "Elements of Chemical Philosophy."

Sir Humphry, adverse to all significant arrangements which may induce erroneous inferences, or obstruct the progress of free inquiry, separates the parts of this volume into divisions, of which he makes seven. An Introduction, embracing an "historical view of the progress of chemistry," although brief, manifests research and the hand of a master. Here, however, we find the philosophy of chemistry accurately defined, and more particularly the application of chemical philosophy happily elucidated with the characteristic animation of the author. The foundations of chemical philosophy being observation, experiment, and analogy, they are respectively illustrated by example, and from their application to some particular substance a general scientific truth is established. Till modern times, these principles of research and combination of methods were never applied; and in the early ages, all that deserves the appellation of chemical consists in observation, with very little analogy and no experiment. The dyeing and works in metals practised by the ancients were merely mechanical; were rather habits occasioned by necessity, than arts aided by reason. Sir H. searches the writings of the Greeks and Romans for chemical knowledge, but finds none;

none; neither Hippocrates, Galen, nor Dioscorides, were acquainted even with distillation. The origin of chemistry, as a science of experiment, cannot be dated further back than the 7th or 8th century of the Christian æra, and is perhaps owing to the Arabians. From that period till the present age, when "it would be indelicate in a cotemporary writer to assume the right of arbitrator on the particular merits of chemical philosophers," we have a brief but interesting sketch of the progress of chemical researches and discoveries. The conclusion of the Introduction unfolds some of the Professor's general views.

"Whether matter consists of indivisible corpuscles or physical points endowed with attraction and repulsion, still the same conclusions may be formed concerning the powers by which they act, and the quantities in which they combine; and the powers seem capable of being measured by their electrical relations, and the quantities on which they act of being expressed by numbers. In combination, certain bodies form regular solids; and all the varieties of crystalline aggregates have been resolved by the genius of Haüy into a few primary forms. The laws of crystallization, of definite proportions, and of the electrical polarities of bodies, seem to be intimately related, and the complete illustration of their connection, probably, will constitute the mature age of chemistry.

"Complexity almost always belongs to the early epochs of every science; and the grandest results are usually obtained by the most simple means. A great part of the phænomena of chemistry may be already submitted to calculation; its most important truths are capable of extremely simple numerical expressions, which may be acquired with facility by students; and there is great reason to believe, that at no very distant period the whole science will be capable of elucidation by mathematical principles. The relations of the common metals to the bases of the alkalies and earths, and the gradations of resemblance between the bases of the earths and acids, point out as probable a similarity in the constitution of all inflammable bodies; and there are not wanting experiments, which render their possible decomposition far from a chimerical idea. It is contrary to the usual order of things, that events so harmonious as those of the system of the earth should depend on such diversified agents as are supposed to exist in our artificial arrangements; and there is reason to anticipate a great reduction in the number of the undecomposed bodies, and to expect that the analogies of nature will be

found

found conformable to the refined operations of art. The more the phænomena of the universe are studied, the more distinct their connection appears, the more simple their causes, the more magnificent their design, and the more wonderful the wisdom and power of their Author."

Part I. of these Elements is "on the laws of chemical changes, on undecompounded bodies and their primary combinations." Division I. "on the powers and properties of matter, and the general laws of chemical changes," in which "the forms of matter, gravitation, cohesion, heat or calorific repulsion (a new term), chemical attraction, laws of combination and decomposition, electrical attraction and repulsion, with their relations to chemical changes," are investigated in a manner equally original and perspicuous. Division II. treats of "radiant or ethereal matter, its effects in producing the phænomena of vision, of heat, chemical changes, and its motions or affections." Division III. of "empyreal undecompounded substances, or undecompounded substances that support combustion, and their combination with each other;" in which oxygen gas, and chlorine or oxymuriatic gas are considered. Division IV. of "undecompounded inflammable or acidiferous substances not metallic, and their binary combinations with oxygen and chlorine, or with each other." Here the nature and properties of hydrogen, azote, sulphur, phosphorus, carbon and diamond, and boron or the boracic basis, are explained. Division V. is of "metals, their primary combinations with other undecomposed bodies, and with each other." Division VI. of "some substances, the nature of which is not yet certainly known;" these are the fluoric principle, and the amalgam from ammoniacal compounds. The VIIth and last division is devoted to "the analogies between the undecompounded substances, speculations respecting their nature, on the modes of separating them, and on the relations of their compounds."

These divisions are perfectly arbitrary, and may be augmented at pleasure, although each seems to be founded on some general principle, or received association of principles. So far is Sir Humphry hostile to all systems in the present state of our chemical knowledge, except that of Baconian induction. He begins with the "*forms* of matter," (a term much too limited and definite for his other views,) divides them into four classes, as solids, fluids [liquids], elastic fluids or gases, and *ethereal* substances. The latter term he prefers to *imponderable* substances for those "forms [perhaps kinds] of matter, which are known to us only in

their

their states of motion when acting on our organs of sense, or upon other matter, and which are not susceptible of being confined. It cannot be doubted that there is matter in motion in space, between the sun and the stars and our globe, though it is a subject of discussion whether successions of particles be emitted from these heavenly bodies, or motions communicated by them, to particles in their vicinity, and transmitted by successive impulses to other particles. *Ethereal* matter differs either in its nature or in its affections by motion; for it produces different effects; for instance, as radiant heat, and as different kinds of light."

After briefly stating the laws of gravitation and cohesion, the Professor rebuts the notion entertained last century, respecting an instrument of attraction or some unknown matter which serves to impel bodies to each other. He adds, "there is no ground for supposing that matter cannot act at a distance [that is, in a vacuum], and it is absolutely necessary for the explanation of the planetary motions, to suppose space in the universe void of matter." This definition of attraction gives to matter an occult quality, more difficult to conceive, more incredible, than all the speculations about spirit which have hitherto appeared. To suppose that two inanimate and naturally inert bodies can act on each other at a distance and in a vacuum, does not convey any satisfactory knowledge to the mind, and gives to such matter something like intelligence or intellect; but if we suppose the attraction of bodies to be effected by some conducting or connecting medium, by a very attenuated atmosphere, for instance, we at least adhere to experience and the mechanical laws of matter, without any extravagant or incredible suppositions.

Among the qualities of matter, the Professor introduces heat, or what he calls "calorific repulsion." He illustrates the common effects of caloric on matter by reference to numerous experiments, and thus proceeds to state his novel doctrine respecting the non-existence of caloric or matter of heat.

"Heat, or the power of repulsion, may be considered as the *antagonist* power to the attraction of cohesion, the one tending to separate, the other to unite the parts of bodies; and the forms of bodies depend upon their respective agencies. In solids, the attractive force predominates over the repulsive; in fluids and in elastic fluids they may be regarded as in different states of equilibrium; and in ethereal substances the repulsive must be considered as predominating over and destroying the attractive force.

"All

"All the different substances in nature, under certain circumstances, are probably capable of assuming all these forms: thus solids, by a certain increase of temperature, become fluids, and fluids gases; and *vice versâ*, by a diminution of temperature gases become fluids, and fluids solids."

"As attempts have been made to account for attraction, by the supposition of the existence of a peculiar matter, so *calorific repulsion* has been accounted for by supposing a subtile fluid, capable of combining with bodies, and of separating their parts from each other, which has been named the *matter of heat*, or *caloric*.

"Many of the phænomena admit of a happy explanation on this idea, such as the cold produced during the conversion of solids into fluids or gases, and the increase of temperature connected with the condensation of gases and fluids; but there are other facts which are not so easily reconciled to the opinion: such are the production of heat by friction and percussion; and some of the chemical changes which have been just referred to. When the temperature of bodies is raised by friction, there seems to be no diminution of their capacities, using the word in its common sense: and in many chemical changes connected with an increase of temperature, there appears to be likewise an increase of capacity. A piece of iron made red hot by hammering cannot be strongly heated a second time by the same means, unless it has been previously introduced into a fire. This fact has been explained by supposing that the fluid of heat has been pressed out of it, by the percussion, which is recovered in the fire: but this is a very rude mechanical idea: the arrangements of its parts are altered by hammering in this way, and it is rendered brittle. By a moderate degree of friction, as it would appear from Rumford's experiments, the same piece of metal may be kept hot for any length of time; so that, if heat be pressed out, the quantity must be inexhaustible. When any body is cooled it occupies a smaller volume than before: it is evident, therefore, that its parts must have approached towards each other: when the body is expanded by heat, it is equally evident that its parts must have separated from each other. The immediate cause of the phænomena of heat, then, is motion, and the laws of its communication are precisely the same as the laws of the communication of motion.

"Since all matter may be made to fill a smaller volume by cooling, it is evident that the particles of matter must have space between them; and since every body can communicate the power of expansion to a body of a lower temperature, that is, can give an expansive motion to its particles, it is a probable inference that its own particles are possessed of motion; but as there is no change in the position of its parts as long as its temperature is uniform, the motion, if it exist, must be a vibratory or undulatory motion, or a motion of the particles round their axes, or a motion of particles round each other.

"It seems possible to account for all the phænomena of heat, if it be supposed that in solids the particles are in a constant state of vibratory motion, the particles of the hottest bodies moving with the greatest velocity and through the greatest space; that in fluids and elastic fluids, besides the vibratory motion, which must be conceived greatest in the last, the particles have a motion round their own axes, with different velocities, the particles of elastic fluids moving with the greatest quickness; and that in ethereal substances the particles move round their own axes, and separate from each other, penetrating in right lines through space. Temperature may be conceived to depend upon the velocities of the vibrations; increase of capacity, on the motion being performed in greater space; and the diminution of temperature, during the conversion of solids into fluids or gases, may be explained on the idea of the loss of vibratory motion, in consequence of the revolution of particles round their axes, at the moment when the body becomes fluid or aëriform, or from the loss of rapidity of vibration in consequence of the motion of the particles through greater space.

"If a specific fluid of heat be admitted, it must be supposed liable to most of the affections which the particles of common matter are assumed to possess, to account for the phænomena; such as losing its motion when combining with bodies, producing motion when transmitted from one body to another, and gaining projectile motion when passing into free space: so that many hypotheses must be adopted to account for its mode of agency, which renders this view of the subject less simple than the other. Very delicate experiments have been made, which show that bodies when heated do not increase in weight. This, as far as it goes, is an evidence against a specific subtile elastic fluid producing the calorific expansion; but it cannot be considered as decisive, on account of the imperfection of our instruments: a cubical inch of inflammable air requires a good balance to ascertain that it has any sensible weight; and

and a substance bearing the same relation to this, that this bears to platinum, could not perhaps be weighed by any methods in our possession.

"Some arguments have been raised in favour of the existence of a specific fluid of heat, from the circumstances of the communication of heat to bodies in exhausted receivers, and from the manner in which they are affected by this heat: but there are no means known in experimental science of producing a perfect vacuum; even the best Torricellian vacuum must contain elastic matter. The great capacity of such highly rarefied matter is an obstacle to the indication of temperature; but supposing a communication of heat, the laws must be analogous to those of heat communicated to common air. If a long *cylinder* of metal, placed perpendicularly, be heated in the middle, the warmest part will be above, from the ascent of heated particles of the elastic medium; but if a *sphere* be heated in the middle, the hottest portion will be below, as the heated elastic matter must remain longer in contact with the inferior than with the superior portion.

"The laws of the communication of heat, and the philosophy of its effects, are independent of this speculative question, which will again be considered, under new relations, in the part of this work relating to the properties of *ethereal* or *radiant matter*."

[To be continued.]

XXIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY*.

May 28. The President in the chair. A paper by Dr. Chisholm was read, detailing a case of nervous affection and mental derangement relieved by pressure on the carotid arteries, as proposed by Dr. Parry. Large doses of mercury were also given with the view of relieving a supposed liver complaint.

June 4. A paper by Mr. F. Baily, on a new method of calculating the value of Life Annuities, was laid before the Society by Sir Humphry Davy. The author took occasion to notice the labours of Mr. Barrett, of Petworth, who had devoted the greatest part of his life in calculating a very voluminous set of Tables upon the principles above alluded to.

* This short sketch of the labours of the Royal Society in May and June, was omitted for want of room in the preceding number.

A long and important mathematical paper, by Señor Rodriguez, a philosopher in the suite of the late Spanish Ambassador, on the French and English admeasurements of an arc of the meridian, was laid before the Society by Mr. Mendoza Rios. This Spanish mathematician has discovered some minute errors in both French and English surveys, and points out with great modesty their probable source. He thinks that one of the greatest deviations has originated in the manner of taking the astronomical observations by the different trigonometrical surveyors.

June 11.—Papers by Dr. Wollaston, on periscopic glasses; by Mr. John Davy, on fluoric acid; and Jan. 18, a paper by Sir H. Davy, on phosphoric and sulphuric acid, were read; for a short account of which see our former numbers.

A paper, by Mr. Brodie, on animal heat, was also read, June 18, tending to confirm some of the author's remarks on this subject in his former communications. He animadverted on the inadequacy of Black's theory, and the inaccuracy of Crawford's experiments; showed that by artificial respiration animal bodies deprived of the brain cool faster than when left alone, although an equal portion of oxygen is absorbed, and carbon disengaged in the process, as if the animal were living; and hence inferred that the action of the brain and nerves is necessary to the production of animal heat. He took notice of the temperature or state of tortoises, which are said to live after the brain has been completely amputated.

Mr. Home drew up some observations on the stomachs of fowls, made by Sir Joseph Banks at the Cape of Good Hope. The right hon. President, having noticed eagles to pass stones and other substances, was induced to consider that there must be something peculiar in the organization of their stomachs; in consequence of which he engaged Mr. Home to investigate such subjects as might fall under his observation.

After this paper was read, the Society adjourned during the long vacation till November next.

LONDON PHILOSOPHICAL SOCIETY.

Last month the attention of the Society was called to a Lecture on the Stage, by Mr. Reid, which embraced its history from the earliest formation to the present time. He began by tracing it to the mystic dramas of the ancients, of which the Egyptian, acted in the rites of Isis and Osiris, were the first model. The Bacchanalian mysteries,

steries, which succeeded them in Greece, were the source of the Greek Stage. At first a single mime sung hymns to the praise of Bacchus; then a second was added, till at last a chorus was formed, composed of many individuals, but uninterrupted by dialogue or pantomime; and this for many years was the only dramatic entertainment known in Greece. At length, Thespis introduced a new personage, who relieved the chorus by reciting a part of some history or fable; and this was called an Episode. From this time the chorus, which at first composed the principal, became insensibly and progressively the most inconsiderable part of the drama. The renowned Æschylus, whose sword and pen have been equally entwined with laurels, and who has been justly called the father of the Greek Tragedy, added a second actor to the first of Thespis, and thus introduced dialogue, which perpetually incroaching on the ancient chorus, has at last driven it from the Stage. It is to this great man that the Athenians were indebted, said the Lecturer, for the first dawn of that refinement which augured and produced so brilliant a day for Greece. He took upon himself the whole management of the drama, brought his actors into a regular theatre, raised his heroes on the cothurnus, and by the introduction of splendid habits imparted an air of greater dignity to his performance. Sophocles and Euripides carefully studied the plan laid down by Æschylus; and, refining upon it with the enthusiasm of genius chastised by judgement, they brought the Stage to its highest state of perfection. After discriminating very ably between the opposite characteristics of Sophocles and Æschylus, the Lecturer proceeded to observe, that comedy owed its birth to the same fortuitous circumstances as tragedy—but in one case the chorus was burlesque, and in the other serious. The old comedy was distinguished into three æras, denominated the old, the middle, and the new. The old retained strong marks of its original rudeness, and of the low abuse and scurrilous jokes vented on the bystanders from the court of Thespis. It was a representation of real actions, and exhibiting the dress, manners, and age of living characters, not even concealing their names; and these were the features of the muse of Aristophanes. The interference of the magistracy to prevent this abuse of the Stage produced the middle comedy, which differed in nothing from the first than by making an attack upon real characters under fictitious names: at length this comedy, yielding to the influence of increasing politeness and refinement, was supplanted by the new, which consummated

summated the purification of the Stage, fostering imagination without offending nature, and displaying character without polluting it by personality. It was in the midst of this gradual refinement of the Athenian Stage that Menander rose to form a purer model of chaste and delicate comedy. A few fragments of his works, saved from the tooth of time, are the only specimens of his style that have reached our age; but these are sufficient to confirm the loss which morality and taste have suffered by the annihilation of the rest.

[To be continued.]

IMPERIAL INSTITUTE OF FRANCE.

[Continued from vol. xxxix. p. 401.]

OPTICS.

New Inquiries of Messrs. Malus and Arago.

A direct ray of light, as is well known, possesses the property of dividing itself into two distinct bundles in its passage through a rhomboid of Iceland spar, whatever in other respects may be its position in relation to the principal section of the rhomboid.

If we subject the light of which one of these bundles is composed to the action of a second rhomboid, we find that it differs essentially from the direct light, since, in certain positions of the principal section of the second crystal, it no longer undergoes double refraction: for the discovery of this beautiful property we are indebted to Huyghens.

When endeavouring to account for this experiment, Newton remarks, in one of the questions which he has placed at the end of his treatise on Optics, that it is necessary to admit that the molecules of which the luminous rays are composed must have sides endowed with different properties: these sides, which some authors have designated by the appellation of poles, are diametrically opposite to each other, and in two directions respectively rectangular.

This being granted, in a ray of ordinary light, the poles of the molecules will not affect any particular position, and will be uniformly directed towards all the points of the space; whereas a polarized ray will be composed of molecules, the similar poles of which will have the same situation: this last ray will be distinguished from a ray of direct light, in so far as the latter is always divided into two fasciculi in its passage through a rhomboid of carbonate of lime; while the polarized ray experiences only a single refraction

fraction in some particular positions of the principal section of the crystal to which we present it.

The polarized rays differ from the rays of direct light by several other properties which were unknown to Huyghens and to Newton, and the discovery of which is owing to M. Malus. If we suppose in fact, that after having disposed vertically the principal section of a rhomboid of carbonate of lime, we receive the two fasciculi which proceed from them on the surface of smooth water, and under an angle of 52° 45′, we shall remark that the common fasciculus acts like the direct ray; since it abandons to the partial reflection a part of its molecules: as to the extraordinary fasciculus, it penetrates the liquid quite through. If we suppose, on the contrary, that the principal section of the rhomboid is perpendicular to the plane of incidence, the extraordinary ray undergoes the partial reflection, and the ordinary ray penetrates the liquid entirely.

When we examine, by the help of a rhomboid of calcareous spar, the light which is reflected on the surface of the water, and under an angle of 42° 45′, we see that it has all the characters of one of the fasciculi produced by the double refraction of a crystal, for it no longer constantly separates into two fasciculi: in this experiment, which is in some measure the converse of that which we have first referred to, the plane of reflection performs the office of the principal section of the first rhomboid. We have only explained these results (which are detailed at great length in the elegant work by M. Malus, to which the Class decreed the prize of Mathematics for 1810) in order to indicate the point from which the members of the Class set out, who were occupied with this object in 1811.

We have hitherto only alluded to the modifications which the luminous rays undergo in their reflection. Is the light transmitted by diaphanous bodies, even modified in certain circumstances which we are about to mention?

If we place two object glasses, one above the other, there are formed, as we all know, some coloured rings, of which the point of contact is the common centre: these rings are perceived either by the aid of reflected or of transmitted light. When the angle of the rays transmitted with the surface of the object-glass is about 32°, they are polarized, since in certain positions of the principal section of a crystal of Icelandic spar, we see only a single image of rings. Now, it is a very remarkable circumstance in this experiment, that the modification which the rays forming the rings undergo in passing through the object-

object-glasses, is entirely identical with that which the reflection communicates to them; so that, for instance, if in a determinate position of the object-glasses and of a crystal, when we look at the reflected rings, we only perceive the image of the rings which proceed from the extraordinary refraction, it will still be the extraordinary image which we shall perceive, when in similar circumstances we shall examine the rings transmitted. This result, which M. Arago communicated to the Class in the month of February, seems to prove that the coloured rings are formed solely at the expense of the light, which in the presence of the second lens will be partially reflected, and thus establishes an intimate connection between these two most extraordinary classes of the phænomena of optics.

On the 11th of March, M. Malus announced to the Class, that on subjecting, at various times, the light which the glasses transmit under an angle of 35° 25′, he had ascertained that this light is composed of a certain quantity of rays polarized in a direction contrary to the reflected rays, and of another portion of rays not modified, which preserve the properties of the direct light: this last portion diminishes at each new transmission of the fasciculus; so that, if we pass through a pile of parallel glasses, the portion of light transmitted is entirely polarized in one direction, while the rays successively reflected are polarized in a contrary direction. M. Malus concludes from this, that at all times, when by any contrivance we produce a ray polarized in one direction, we necessarily obtain a ray polarized in a direction diametrically opposite, and that these rays follow different routes. The observation of M. Arago, which we have recently mentioned, forms the only exception to this general rule, since the rings reflected and transmitted are polarized in the same manner.

M. Arago had long ago ascertained that diaphanous and opake bodies modify the light which they reflect: the metallic bodies alone seemed to him to impress no new property. It is true that opticians were well aware that there was a slight difference between the intensity of the two images formed by a rhomboid, by the help of rays reflected by a metallic plane: but this isolated fact could teach us nothing relative to the particular mode of action of the metallic bodies and of light. But in a memoir read to the Institute on the 27th of May 1811, M. Malus has shown, by experiments made on rays already polarized, and by the help of a method of which it would be difficult to give a clear idea in an extract, that the light reflected by the metals

tals contains at once rays polarized in two directions, so that, in its decomposition by a crystal of carbonate of lime, it acts like the ordinary light. Hence it results, that all the bodies in nature polarize the light under determinate angles, and that here and there among these angles the rays receive this modification in a very incomplete manner.

[To be continued.]

LECTURES.

Mr. John Taunton, F.A.S. will deliver a Course of Lectures on Anatomy, Physiology, Pathology, and Surgery.

In this Course of Lectures it is proposed to take a comprehensive view of the structure and œconomy of the living body, and to consider the causes, symptoms, nature, and treatment of surgical diseases, with the mode of performing the different surgical operations; forming a complete course of anatomical and physiological instruction for the medical or surgical student, the artist, the professional or private gentleman.

An ample field for professional edification will be afforded by the opportunity which Pupils may have of attending the clinical and other practice of both the City and Finsbury Dispensaries.

The Autumnal Course will commence on Saturday, October the 3d, 1812, at eight o'clock in the evening precisely, and be continued every Tuesday, Thursday, and Saturday, at the same hour.

Particulars may be had, on applying to Mr. Taunton, Greville-street, Hatton Garden.

St. Thomas's and Guy's Hospitals.—The Winter Course of Lectures at these adjoining Hospitals will begin as usual, October 1, viz. at St. Thomas's, Anatomy and the Operations of Surgery, by Mr. A. Cooper and Mr. Henry Cline.—Principles and Practice of Surgery, by Mr. A. Cooper.

At Guy's.—Practice of Medicine, by Dr. Babington and Dr. Curry.—Chemistry, by Dr. Babington, Dr. Marcet, and Mr. Allen.—Experimental Philosophy, by Mr. Allen.—Theory of Medicine, and Materia Medica, by Dr. Curry and Dr. Cholmeley.—Midwifery, and Diseases of Women and Children, by Dr. Haighton.—Physiology, or Laws of the Animal Œconomy, by Dr. Haighton.—Structure and Diseases of the Teeth, by Mr. Fox.

N.B. These several Lectures are so arranged, that no two of them interfere in the hours of attendance; and, with

with the Lectures on Anatomy, and those on the Principles and Practice of Surgery, given at the Theatre of St. Thomas's Hospital adjoining, the whole is calculated to form a complete Course of Medical and Chirurgical Instruction. Terms and other particulars may be learnt from Mr. Stocker, Apothecary to Guy's Hospital.

Dr. Clutterbuck will begin his Autumnal Course of Lectures on the different branches of Medicine, on Monday, October 5, at ten o'clock in the morning, at his House, No. 1, in the Crescent, New Bridge street, Blackfriars.

The different Lectures will be delivered on the alternate days, at the same hour, viz. on the Theory and Practice of Physic, on Mondays, Wednesdays, and Fridays; and on the Materia Medica and Chemistry, on Tuesdays, Thursdays, and Saturdays, throughout the season.

Mr. Wilson and Mr. Charles Bell will, in future, deliver the Lectures on Anatomy, Physiology, Pathology, and Surgery, conjointly, at the Theatre of Anatomy, Great Windmill-street. The Lectures are so arranged, that the subjects delivered during the Autumnal Course, by the one, will, in the Spring, be given by the other. Each will, therefore, during the season, give one complete Course of Lectures.

The Museum of Windmill-street, now consisting of the united collections of Mr. Wilson and Mr. Bell, may be seen by gentlemen of the profession during the vacation months, by applying to Mr. Bell, 34, Soho Square.

LIST OF PATENTS FOR NEW INVENTIONS.

To John Webb, of Hoxton, Middlesex, weaver, for his method of making rugs, carpets, or any other article of furniture, or dress, &c.—13th June, 1812.

To William Ariell, of the parish of St. Ann, Limehouse, Middlesex, shipwright, for certain improved machines or machinery for extracting corroded iron and other nails and bolts from ships' bottoms, masts, decks, and other parts thereof.—25th June.

To Anthony Senick, of Gracechurch Street, London, merchant, for his improved mode of roasting coffee.—25th June.

To Thomas Cobb the younger, of Calthrop House, near Banbury, in the county of Oxford, paper-maker, for further improvements in the art of making paper in separate sheets. —16th July.

To

To John Simpson, of the parish of Sutton, in the county of York, merchant, for his method of cleaning, gunning, and scouring whalebone.—16th July.

To John Simpson, of Birmingham, in the county of Warwick, tin-plate worker, for his improvements in the construction of lamps, which lamps so improved he denominates "Palmer's Birmingham Economic Lamps."—16th July.

To John Sutherland, of Liverpool, in the county of Lancaster, copper-smith, for his improvement in the construction of copper mills and intermediate condensers.—16th July.

To Morris Tobias, of Wapping, in the county of Middlesex, watch manufacturer, for his binnacle time-piece or time-keeper.—16th July.

To James Walker, of Maidstone Hill, in the county of Kent, gentleman, for his improved tubular metallic vesssel, and the application thereof to the preservation of fluids and various other things.—16th July.

To Tebaldo Monzani, of Old Bond Street, in the county of Middlesex, musical instrument-maker, for certain improvements on clarinets and German flutes.—16th July.

To Thomas Motley, of the city of Bristol, iron-monger, for his improved method of manufacturing letters or characters for signs, show-boards, fronts of shops, houses, and other places, and for any other purpose of composing and indicating names or words in relief in a conspicuous manner.—22d July.

To William Smith, of Lisle Street, in the county of Middlesex, gun-maker, for an improved gun and pistol lock.—July 28.

To John Bellingham, of Levens, near Rostevor, in the county of Down, gentleman, for certain improvements in the make and construction of axle trees, for all descriptions of carriages.—July 28.

To Henry Osborn, of Bordesley, near Birmingham, for a new improved machine for turning and levelling various articles made of iron, preparatory to welding and grinding.—3d August.

To John Rapson, of Penryn, in the county of Cornwall, millwright and machine maker, for a new or improved method of communicating a regular or irregular motion from one axle to another, placed at any angle, without the aid of an universal joint.—5th August.

METEORO-

METEOROLOGICAL TABLE,
By Mr. Cary, of the Strand,
For August 1812.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Thermometer. Noon.	Thermometer. 11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
July 27	60	60°	55°	29·60	0	Rain
28	58	68	56	·55	36	Showery
29	58	69	52	·56	37	Showery
30	56	68	55	·85	60	Fair
31	56	67	52	·88	62	Fair
August 1	56	66	53	·78	0	Rain
2	57	67	55	·82	31	Fair
3	57	64	55	·76	0	Rain
4	56	62	55	·78	20	Showery
5	56	56	51	·80	0	Rain
6	52	60	52	·83	26	Showery
7	51	63	51	·86	38	Fair
8	51	59	51	·88	10	Showery
9	54	60	52	·92	32	Showery
10	53	59	52	·88	30	Cloudy
11	54	62	54	·96	0	Rain
12	54	60	53	30·08	42	Fair
13	54	69	52	·07	67	Fair
14	53	73	56	·17	62	Fair
15	56	69	57	·10	52	Fair
16	57	70	64	·02	47	Cloudy
17	64	74	66	29·99	70	Fair
18	66	76	67	·88	80	Fair
19	67	75	60	·75	62	Fair
20	61	70	66	·95	71	Fair
21	64	70	60	·90	42	Showery
22	64	69	62	·97	60	Fair
23	64	70	64	·90	53	Fair
24	66	69	54	·97	47	Fair
25	57	70	55	·90	48	Fair
26	56	69	56	·89	45	Fair

N. B. The Barometer's height is taken at one o'clock.

XXX. *A New Table of the Quantities of Acid in Sulphuric Acid of different Densities, constructed for the Use of Manufacturers; being the Result of Experiments made with the strongest Sulphuric Acid of Commerce. The specific Gravities taken at the Temperature of 60°. By* Samuel Parkes, *F.L.S.*

To Mr. Tilloch.

Sir, Having been applied to by several persons in different parts of the kingdom, for instructions respecting a mode of ascertaining the quantity of acid in diluted sulphuric acid, I invariably referred them to Dr. Kirwan's Table, which was published in the year 1793 in the fourth volume of the Transactions of the Royal Irish Academy. This however always proved unsatisfactory, inasmuch as few manufacturers understood how to accommodate that table to the common oil of vitriol, or make it applicable to any practical purpose. This has arisen from the circumstance of Dr. Kirwan having taken sulphuric acid, as it exists in sulphate of potass, instead of the strongest oil of vitriol, for his standard of real acid. Having, in my own business, found the inconvenience of this, I several years ago formed a Table for my private use, from the sulphuric acid of commerce, by diluting that acid with several successive portions of water; a table which I have ever since employed with great advantage. But as this table was formed at a temperature above the mean temperature of the atmosphere, and its range was too confined to be of use in every instance, I resolved to undertake the formation of a new one that would be more generally useful, and that should descend by regular gradations down to the lowest point of dilution that any consumer of sulphuric acid would be likely to require *. Having now, at no small expense of time and labour, completed this Table, I shall be obliged if you will have the goodness to insert it in the next Philosophical Magazine, as I conceive that it will be acceptable and useful to many individuals in various branches of trade and manufacture.

In making this table, I might have taken *purified* sulphuric acid, *i. e.* such as had undergone a second distillation; but as this is an article which is never employed but for philosophical purposes, I thought it better to make use of the

* The first line of the Table shows the specific gravity of a mixture of one part water and 100 parts of strong sulphuric acid. The latter, the specific gravity of one part of sulphuric acid mixed with 100 parts of water.

common acid, and to take as good a sample of that kind as I could possibly procure. I have therefore chosen some of my own manufacture, made in the usual way, and concentrated, as is common in large manufactories, by steaming it in a boiler of lead, and finishing it in a retort of glass. When I began the experiment, the atmosphere of the room was at 60°*, and the acid was of the specific gravity of 1·8494, which, at this temperature, is as strong as it is ever sold.

In order to form the annexed Table, I proceeded as follows:

I first accurately weighed ten thousand grs. of this acid into a stoppered bottle of glass, and then added to it 100 grains of pure water †. When the mixture was become cool, after having been sufficiently agitated, the specific gravity of it was taken, and the result forms the first line of the Table. In this way I continued to dilute the acid with successive portions of water, taking care to let it rest a sufficient time between each addition of the water, that a complete union between the acid and the water might take place; having found by experiment that a mixture of sulphuric acid and water, even after it has become cold, requires several hours for it to arrive at the maximum of condensation. I took also the precaution of keeping it in stoppered bottles, that it might not imbibe water from the atmosphere, and frequently agitated it during each interval. And, in order to attain as great accuracy as possible, I procured a gravity bottle larger than usual, one that holds nearly ten ounces of water, and with a stem so small that at the part to which the fluid rises when properly filled, a single drop of the liquor will occasion a rise of nearly one sixteenth of an inch.

The balance which I made use of in these experiments, is so delicate that it will turn with the 20th part of a grain; and as the acids will erode the common scale-dishes, if dropt upon them, and endanger the accuracy of a result, I have long been in the habit of using those made with Wedgwood's ware, and such were employed in making this Table. Those parts of the Table which have a star opposite to them, were made by actual experiment—the intermediate ones were the results of calculation.

* For the difference which is occasioned in the specific gravity of sulphuric acid by change of temperature, consult Nicholson's Journal, quarto, vol. iii. page 211.

† In diluting sulphuric acid, it is usual, and safer, to add the acid by degrees to the water, and not the water to the acid; but with this very small quantity of water, such caution was not necessary.

Since

Since I concluded these experiments it occurred to me, that it might be useful to some manufacturers to know the quantity of acid per cent. in diluted acid of any given strength; I have therefore calculated the quantity in each, and annexed it. The column of *ounces* and *drachms* is added for the use of those who are accustomed to this mode of reckoning, and also for those who are not conversant with the usual method of stating specific gravities; and is calculated on the supposition of the wine pint holding exactly sixteen ounces avoirdupois of pure water*.

The sulphuric acid which is consumed in these kingdoms amounts I believe to upwards of three thousand tons annually; the greater part of which is used in a state of dilution. For the purposes of dissolving iron or zinc, it should be diluted with at least five or six times its weight of water. Sulphuric acid is consumed in large quantities by bleachers, for making the oxymuriate of lime, and these people always use it in a state of dilution. The calico printers also expend large sums in the purchase of this acid, which they use in various states of dilution, for making what they call sours.

To these, and other manufacturers, this Table will be of use, not only in assisting them in the formation of acid of any given strength, but it will enable them at any time to ascertain whether their servants have observed due care in making the different preparations; which is a matter of great moment—for it often happens, that for want of this the printer and manufacturer suffer great loss, and the goods sustain an irreparable injury.

I need scarcely add that the oil of vitriol-makers themselves, may also derive great benefit from attending to this Table.

I am, sir, yours, &c.

Goswell-street Chemical Works, London, Sept. 2, 1812. SAMUEL PARKES.

* It is a convenient way of measuring, to have a glass bottle with a narrow neck, holding rather more than a pint of water, with a mark on the stem exactly at the place to which the water ascends when it contains exactly 16 ounces avoirdupois of that fluid. It was formerly understood that the standard wine pint was the exact measure of 16 ounces of pure water; but some experiments made in the year 1688, before the Lord Mayor and the Board of Excise, decided, that the sealed wine gallon of Guildhall contains only 224 cubic inches, though by the fifth of Queen Ann, chap. vii. § 7, it is called 231 inches. This decision makes the wine pint to hold only ·970 of an avoirdupois pound of water; but as it is generally thought right, for all common purposes still to consider it of the capacity of 16 ounces avoirdupois, I have taken it at that in this instance.

A Table of the Specific Gravities of Sulphuric Acid, when diluted with different Portions of Water, at the Temperature 60°.

	Drachms of Water.	Specific Gravity.	Weight of the Wine Pint.		Acid per Cent.
			Oz.	Drms.	
100 Drachms Sulp. Acid. Sp. Grav. 1·8494, Or, 29 oz. 9½ Drachms, the Weight, Avoirdupois, of the Wine Pint.	* 1	1·8484	29	9¼	99·009
	* 2	1·8465	29	8¾	98·039
	* 3	1·8445	29	8¼	97·087
	* 4	1·8416	29	7½	96·153
	* 5	1·8387	29	6¾	95·238
	6	1·8358	29	6	94·339
	* 7	1·8319	29	5	93·457
	8	1·8270	29	3¾	92·509
	9	1·8222	29	2½	91·743
	*10	1·8163	29	1	90·909
	11	1·8104	28	15½	90·090
	12	1·8046	28	14	89·285
	13	1·7988	28	12½	88·495
	14	1·7929	28	11	87·719
	*15	1·7880	28	9¾	86·956
	16	1·7821	28	8	86·206
	17	1·7744	28	6¼	85·470
	18	1·7666	28	4¼	84·745
	19	1·7588	28	2¼	84·033
	20	1·7510	28	0¼	83·333
	*21	1·7431	27	14¼	82·644
	22	1·7353	27	12¼	81·967
	23	1·7275	27	10¼	81·300
	24	1·7207	27	8½	80·645
	25	1·7138	27	6¼	80·000
	26	1·7070	27	5	79·365
	*27	1·7002	27	3¼	78·740
	28	1·6933	27	1½	78·125
	29	1·6865	26	15¾	77·519
	30	1·6796	26	14	76·923
	31	1·6728	26	12¼	76·335
	32	1·6660	26	10½	75·757
	*33	1·6582	26	8½	75·187
	34	1·6523	26	7	74·626
	35	1·6464	26	5½	74·074
	36	1·6406	26	4	73·529
	37	1·6348	26	2½	72·992

	Drachms of Water.	Specific Gravity.	Weight of the Wine Pint.		Acid per Cent.
			Oz.	Drms.	
100 Drachms	38	1·6289	26	1	72·463
Sulp. Acid.	39	1·6230	25	15½	71·942
Sp. Grav.	*40	1·6171	25	14	71·428
1·8494,	41	1·6113	25	12½	70·921
Or, 29 oz.	42	1·6054	25	11	70·422
9½ Drachms,	43	1·5995	25	9½	69·930
the Weight,	44	1·5937	25	8	69·444
Avoirdupois,	*45	1·5879	25	6½	68·965
of the Wine	46	1·5820	25	5	68·493
Pint.	47	1·5761	25	3½	68·027
	48	1·5703	25	2	67·567
	49	1·5645	25	0½	67·114
	50	1·5585	24	15	66·666
	51	1·5526	24	13½	66·225
	52	1·5478	24	12¼	65·789
	53	1·5429	24	11	65·359
	*54	1·5390	24	10	64·935
	55	1·5351	24	9	64·516
	56	1·5312	24	8	64·102
	57	1·5273	24	7	63·694
	58	1·5234	24	6	63·291
	59	1·5195	24	5	62·893
	60	1·5156	24	4	62·500
	61	1·5117	24	3	62·111
	*62	1·5078	24	2	61·728
	63	1·5039	24	1	61·349
	64	1·5000	24	0	60·975
	65	1·4960	23	15	60·606
	66	1·4921	23	14	60·240
	67	1·4882	23	13	59·880
	68	1·4843	23	12	59·523
	69	1·4804	23	11	59·171
	*70	1·4765	23	10	58·823
	71	1·4726	23	9	58·481
	72	1·4687	23	8	58·139
	73	1·4648	23	7	57·803
	74	1·4609	23	6	57·471
	75	1·4570	23	5	57·142
	76	1·4531	23	4	56·818
	77	1·4502	23	3¼	56·497
	*78	1·4473	23	2½	56·179

	Drachms of Water	Specific Gravity.	Weight of the Wine Pint. Oz.	Drms.	Acid per Cent.
100 Drachms Sulp. Acid. Sp Grav. 1·8494, Or. 29 oz. 9½ Drachms, the Weight, Avoirdupois, of the Wine Pint.	79	1·4433	23	1½	55·865
	80	1·4395	23	0½	55·555
	81	1·4365	22	15¾	55·248
	82	1·4336	22	15	54·945
	83	1·4306	22	14¼	54·644
	84	1·4276	22	13½	54·347
	85	1·4257	22	12¾	54·054
	*86	1·4218	22	12	53·763
	87	1·4189	22	11¼	53·475
	88	1·4160	22	10½	53·191
	89	1·4130	22	9¾	52·910
	90	1·4101	22	9	52·631
	91	1·4072	22	8½	52·356
	92	1·4042	22	7½	52·083
	93	1·4013	22	6¾	51·813
	*94	1·3984	22	6	51·546
	95	1·3955	22	5¼	51·282
	96	1·3926	22	4½	51·020
	97	1·3906	22	4	50·761
	98	1·3886	22	3½	50·505
	99	1·3867	22	3	50·256
	*100	1·3848	22	2½	50·000
	105	1·3730	21	15½	48·780
	*110	1·3632	21	13	47·619
	115	1·3535	21	10½	46·511
	*120	1·3437	21	8	45·454
	125	1·3359	21	6	44·444
	*130	1·3281	21	4	43·478
	135	1·3203	21	2	42·553
	*140	1·3125	21	0	41·666
	145	1·3056	20	14¼	40·816
	*150	1·2988	20	12½	40·000
	155	1·2919	20	10¾	39·215
	*160	1·2851	20	9	38·461
	165	1·2783	20	7¼	37·735
	170	1·2724	20	5¾	37·037
	175	1·2676	20	4½	36·363
	*180	1·2627	20	3¼	35·714
	185	1·2568	20	1¾	35·087
	190	1·2520	20	0½	34·482
	195	1·2470	19	15¼	33·898

	Drachms of Water.	Specific Gravity.	Weight of the Wine Pint.		Acid per Cent.
			Oz	Drms.	
100 Drachms Sulp. Acid. Sp. Grav. 1·8494, Or, 29 oz. $9\frac{1}{2}$ Drachms, the Weight, Avoirdupois, of the Wine Pint.	*200	1·2421	19	14	33·333
	210	1·2343	19	12	32·258
	*220	1·2265	19	10	31·250
	230	1·2187	19	8	30·303
	*240	1·2129	19	$6\frac{1}{2}$	29·411
	250	1·2060	19	$4\frac{3}{4}$	28·571
	*260	1·1992	19	3	27·777
	270	1·1933	19	$1\frac{1}{2}$	27·027
	*280	1·1875	19	0	26·315
	290	1·1825	18	$14\frac{3}{4}$	25·641
	*300	1·1776	18	$13\frac{1}{2}$	25·000
	310	1·1728	18	$12\frac{1}{4}$	24·390
	320	1·1679	18	11	23·809
	330	1·1630	18	$9\frac{3}{4}$	23·255
	*340	1·1582	18	$8\frac{1}{2}$	22·727
	350	1·1552	18	$7\frac{3}{4}$	22·222
	360	1·1523	18	7	21·739
	370	1·1494	18	$6\frac{1}{4}$	21·276
	*380	1·1464	18	$5\frac{1}{2}$	20·833
	390	1·1426	18	$4\frac{1}{2}$	20·408
	400	1·1338	18	$2\frac{1}{4}$	20·000
	*420	1·1328	18	2	19·230
	440	1·1279	18	$0\frac{3}{4}$	18·518
	*460	1·1240	17	$15\frac{3}{4}$	17·857
	480	1·1181	17	$14\frac{1}{4}$	17·241
	*500	1·1132	17	13	16·666
	*550	1·1054	17	11	15·384
	*600	1·0966	17	$8\frac{3}{4}$	14·285
	*650	1·0898	17	7	13·333
	*700	1·0839	17	$5\frac{1}{2}$	12·500
	*750	1·0781	17	4	11·764
	*800	1·0732	17	$2\frac{3}{4}$	11·111
	*850	1·0693	17	$1\frac{3}{4}$	10·526
	*900	1·0664	17	1	10·000
	*950	1·0625	17	0	9·523
	*1000	1·0602	16	$15\frac{1}{2}$	9·090
	*1100	1·0546	16	14	8·333
	*1200	1·0507	16	13	7·692
	*1300	1·0488	16	$12\frac{1}{2}$	7·142
	*1400	1·0458	16	$11\frac{3}{4}$	6·666
	*1500	1·0429	16	11	6·250

	Drachms of Water.	Specific Gravtiy.	Weight of the Wine Pint.		Acid per Cent.
			Oz.	Drms.	
100 Drachms Sulp. Acid. Sp. Grav. 1·8494, Or, 29 oz. $9\frac{1}{2}$ Drachms, the Weight, Avoirdupois, of the Wine Pint.	1600	1·0390	16	10	5·882
	1700	1·0370	16	$9\frac{1}{2}$	5·555
	*1800	1·0351	16	9	5·263
	1900	1·0337	16	$8\frac{5}{8}$	5·000
	*2000	1·0322	16	$8\frac{1}{4}$	4·761
	*2250	1·0283	16	$7\frac{1}{4}$	4·255
	*2500	1·0254	16	$6\frac{1}{2}$	3·846
	*2750	1·0234	16	6	3·508
	*3000	1·0214	16	$5\frac{1}{2}$	3·225
	*3500	1·0185	16	$4\frac{3}{4}$	2·777
	*4000	1·0166	16	$4\frac{1}{4}$	2·439
	*4500	1·0146	16	$3\frac{3}{4}$	2·173
	*5000	1·0127	16	$3\frac{1}{4}$	1·960
	5500	1·0117	16	3	1·785
	*6000	1·0107	16	$2\frac{3}{4}$	1·639
	6500	1·0102	16	2 [illegible]	1·515
	*7000	1·0098	16	$2\frac{1}{2}$	1·408
	7500	1·0093	16	$2\frac{3}{8}$	1·315
	*8000	1·0088	16	2 [illegible]	1·234
	8500	1·0083	16	$2\frac{1}{8}$	1·162
	*9000	1·0078	16	2	1·098
	9500	1·0073	16	$1\frac{7}{8}$	1·041
	*10000	1·0068	16	$1\frac{3}{4}$	0·990

XXXI. *An Account of some Experiments on the Combinations of different Metals and Chlorine, &c.* By JOHN DAVY, *Esq. Communicated by Sir* HUMPHRY DAVY, *Knt. LL.D. Sec. R. S.*

[Concluded from p. 91.]

5. *On the Relation between the Proportion of Oxygen and Chlorine in Combination with several Metals.*

ERRORS being very common in chemical analyses, even in those conducted most skilfully and carefully, all possible means should be taken to discover them; and no means, I think, promise to be more effectual for this purpose, than the general analogy of definite proportions. From a great variety of facts, it appears that oxygen and chlorine combine with bodies in the ratio of 7·5 to 33·6. With one part

part by weight of hydrogen, for example, 7·5 of oxygen unite to form water, and 33·6 of chlorine unite with the same proportion to produce muriatic acid gas. To judge therefore of the accuracy of the analyses of the preceding combinations of the metals and chlorine, it is only necessary to compare them with the analyses of the oxides of the same metals. If the two agree, there will be reason to consider them both correct; but should they disagree, there is equal reason for supposing one or both of them to be wrong.

Thus, as the orange oxide of copper is analogous to cuprane and the brown oxide to cupranea, the oxygen and chlorine should be to each other in these compounds as 7·5 to 33·6. And from comparison of my analysis, with those of Mr. Chenevix and M. Proust, it appears, that in the two first, copper being as 60, the oxygen is to the chlorine as 7·79, instead of 7·5 to 33·77, instead of 33·6; and in the two last as 7·5 to 33·6, or as 15 to 67·2. Coincidences as near as might be reasonably expected.

There is not the same agreement between M. Proust's analyses of the oxides of tin and the preceding ones of the combinations of this metal and chlorine. This discordance induced me to repeat my analyses; and obtaining the same result as at first, I directed my attention to the oxides of tin, and made the following experiments to ascertain the proportion of their constituent parts.

42·5 grains of tin, which had been precipitated from the muriat of this metal by zinc, were heated with nitric acid in a platina crucible, and slowly converted into peroxide; the acid and water were driven off by gentle evaporation at first, and afterwards by a strong red heat continued for a quarter of an hour. The peroxide thus produced was of a light yellow colour; and being very gradually dried, it was semi-transparent, and hard enough to scratch glass; it weighed 54·25 grains. Hence, as 42·5 grains of tin acquire, on conversion into peroxide, 11·75 grains of oxygen, this oxide appears to contain 21·66 per cent. of oxygen, just the quantity found in the native oxide by Klaproth, instead of 28, the proportion stated by Proust.

M. Berthollet, jun. has shown that M. Proust's estimate of 20 per cent. of oxygen in the protoxide is incorrect. To ascertain the true proportion, 20 grains of tin were dissolved in strong muriatic acid in a retort connected with a pneumatic apparatus, and without the assistance of heat; 16 cubic inches of hydrogen gas were produced. (Barom. 30, thermom. 60.) As the production of this quantity of hydrogen

hydrogen indicates an absorption of oxygen by the tin equivalent to eight cubic inches, or (as 100 cubic inches weigh 34·2 grains) to 2·736 grains, the protoxide of tin appears to contain 11·99 per cent. of oxygen.

These analyses of the oxides, compared with those of the combinations of tin and chlorine, are found very nearly to agree. The ratio of oxygen to chlorine in the two first similar compounds, the tin being as 55, is as 7·5 to 33·4; and in the two last, viz. the peroxide and the liquor of Libavius, as 7·6 to 33·5, or as 15·2 to 67.

As the black oxide of iron is formed by the decomposition of ferrane by a solution of potash, and the red oxide by that of ferranea, it is evident that these oxides and combinations of iron and chlorine should coincide in the proportions of their constituent parts. This appears from the analyses* of Dr. Thomson to be nearly the case; for iron being as 29·5, the oxygen is to the chlorine in the black oxide and ferrane as 8 instead of 7·5 to 33·6; and in the two others as 8 to 33·6, or as 13·2 to 55·5. Here the agreement is less than in other instances; but this is not surprising considering the different estimates of the proportions of oxygen in the oxides of iron, and the difficulty of ascertaining them correctly.

The yellow oxide of lead and the white oxides of antimony, bismuth, zinc, and arsenic are formed, when the combinations of these metals and chlorine are decomposed by a solution of potash. But on comparison with the best analyses of the oxides, there is not, excepting in the case of zinc and arsenic, that coincidence of proportions which might be expected. Zinc being as 34·5, the oxygen in the oxide, from the analysis of Proust, is to the chlorine as 7·5 to 34·4; and the arsenic being as 21·9, the oxygen, from the analysis of the same chemist, is to the chlorine as 7·3 to 33·6. The analyses of the oxides of the other metals being at variance with those of the chlorine combinations, I was induced to make the following experiments, with the hope of discovering the cause of the difference.

100 grains of lead, which had been precipitated from the nitrat of lead by zinc, were dissolved in nitric acid and thrown down by carbonat of potash. This precipitate of carbonat of lead was well washed, and dried and heated to dull redness for a quarter of an hour in a platina crucible; by this treatment all the carbonic acid was expelled; the remaining yellow oxide weighed 107·7 grains, and it dis-

* Nicholson's Journal, vol. xxvii. p. 375.

solved

solved in muriatic acid without effervescing, and without affording any residue of brown oxide. Hence the yellow oxide of lead appears to contain 7·15 per cent. of oxygen. And this proportion of oxygen in the oxide compared with that of chlorine in plumbane, lead being as 97·2, appears to be in the ratio of 7·5 to 33·8, instead of that of 15·6 the estimate of Klaproth, or of 11·2 the estimate of Dr. Thomson, to 33·8. Klaproth might have been misled by considering the hydrated oxide as a true white oxide free from water.

According to M. Proust the peroxide of antimony contains 23 per cent. of oxygen, and the protoxide 18*. I have repeated this chemist's experiments; my results, in which the peroxide is concerned, agree with his; but there is not the same concordance in those relating to the protoxide. The protoxide I used was either prepared by the decomposition of the butter of antimony, or of the sulphat, by a boiling solution of carbonat of potash. This oxide, in its purest state, I have always found, as M. Proust describes it, of a light fawn colour before fusion, and afterwards in mass of a gray colour, and of a radiated crystalline texture. 100 grains of it that had been fused were heated in the state of powder with strong test nitric acid in a platina crucible; when nitrous gas ceased to be produced, the excess of nitric acid was expelled by a gentle heat, and the oxide was heated to dull redness, the increase of weight after this was equal to 10·4 grains; nitric acid was again added and the process repeated, but without any alteration of weight being produced. Hence, as the peroxide contains 23 per cent. the protoxide seems to contain 15 per cent.; which proportion of oxygen very nearly agrees with that of chlorine in the butter of antimony; for antimony being as 42·5, the former is to the latter as 7·5 to 34·6 instead of 33·6. I put some confidence in this estimate of the proportion of oxygen in the protoxide, not only on account of its agreement with the analysis of the butter of antimony, but because it was confirmed on the repetition of the experiment.

Klaproth concludes from his experiments, that the oxide of bismuth, prepared by means of nitric acid, contains 17·7 per cent. of oxygen, and in consequence this oxide has been considered distinct from that which is formed by direct calcination of the metal, and which contains a much smaller proportion. But there is reason to believe that this dif-

* *Journal de Physique*, tom. lv.

ference

ference does not really exist, and that there is only one known oxide of bismuth, and that Klaproth's oxide was an hydrated oxide; for I have found that 100 grains of bismuth, converted by nitric acid into oxide, precisely in the same manner as the protoxide of antimony was more highly oxidated, gained only 11·1 grains. Klaproth did not heat his oxide to redness, and hence apparently the discordance. From the above result, which I have confirmed by repetition of the experiment, oxide of bismuth seems to contain 10 per cent. of oxygen, and bismuth being as 67·5, the oxygen in the oxide is to the chlorine in the butter of bismuth as 7·5 to 34·2.

6. *On the Relation between the Proportion of Sulphur in the Sulphurets, and the Proportion of Chlorine in some of the Combinations of Chlorine and the Metals.*

The last section afforded proofs of the useful application of the general analogy of definite proportions in correcting the results of chemical analyses. In the present section, it is my intention to pursue a little further, the plan that I have adopted in the preceding, and to apply another test to the analyses of the combinations of the metals and chlorine, by comparing some of them with the combinations of the same metals and sulphur.

I was first led to examine the sulphurets of tin on a different account. Aurum musivum, it has been observed, is formed when stannane is heated with sulphur. According to M. Proust, this substance is a sulphuretted oxide of tin. Were this opinion correct, an argument might evidently be deduced from it, in favour of the existence of oxygen in chlorine. To satisfy myself respecting this, I endeavoured to ascertain whether any sulphureous acid gas is produced by the decomposition of aurum musivum by heat, as it is commonly asserted. I heated to redness in a bent luted green glass tube connected with a pneumatic mercurial apparatus about 20 grains of aurum musivum, prepared by the decomposition of stannane with sulphur; no more gas was produced than the expansion by heat occasioned, sulphur sublimed, and a gray sulphuret of tin remained. These results I have several times obtained, and not only with aurum musivum prepared as the preceding, but with some also made according to Woulfe's process. As no sulphureous acid gas was produced, and as sulphur sublimed, it may be concluded that aurum musivum differs merely from the gray sulphuret in containing a larger quantity of sulphur. My next object was to ascertain the exact

exact proportion of sulphur in both these sulphurets, for the sake of comparison with the combinations of tin and chlorine.

100 grains of tin in a finely divided state, as precipitated from the muriat of this metal by zinc, were heated in a glass tube intimately mixed with sulphur; the combination of the two was accompanied with vivid ignition, the sulphuret formed weighed 127·3 grains, and broken, it appeared perfectly homogeneous; it was pounded, and again heated with sulphur; but the excess of sulphur being expelled, the fused sulphuret had not increased in weight. The second time I made this experiment, I obtained the same result.

Fifty grains of aurum musivum, purified from mixed sulphur by exposure in a close vessel to a dull red heat, were decomposed by a bright red heat in a small green glass tube nicely weighed, and having only a very small orifice; the loss of sulphur, by conversion into the gray sulphuret, was equal to 9·3 grains. Hence, as 40·7 grains of gray sulphuret contain 8·72 grains of sulphur, 50 grains of aurum musivum appear to contain 18·02 grains.

The ratio in which sulphur combines with bodies is to that in which oxygen and in which chlorine combine, as 15 to 7·5 and 33·6. This appears from the proportions of the constituent parts of sulphuretted hydrogen and sulphureous acid gas, for I have found 100 cubic inches of the former to weigh 36·64 grains, and 100 of the latter 68·44 grains. In the comparison, therefore, between the sulphurets of tin and the combinations of this metal and chlorine, 15 by weight of sulphur are equivalent to 33·6 of chlorine. And the tin being as 55, it appears from the analysis of the gray sulphuret and stannane, that the sulphur is to the chlorine as 15 exactly to 33·4; and from the analysis of the other two compounds, aurum musivum and the liquor of Libavius, as 15·5 to 33·5, or as 31 to 67.

The proportions of sulphur in the two sulphurets of iron do not accord with the proportions of oxygen in the oxides, or of chlorine in the chlorine combinations; but I am yet ignorant of the cause of this difference.

100 grains of lead, heated with sulphur in a glass tube, afforded, in two trials, 115·5 grains of fused sulphuret. Hence lead being as 97·2, the sulphur is to the chlorine in the respective combinations as 15·09 to 33·8.

Sulphuret of antimony contains 25·9 per cent. of sulphur. Hence antimony being as 42·5, the sulphur in the sulphuret is to the chlorine in the butter of antimony, as 14·86 to 34·6.

100

100 grains of bismuth heated with sulphur afforded 122·3 grains of sulphuret. Hence bismuth being as 67·5, the sulphur is to the chlorine as 15·08 to 34·2.

In the following table, the proportions are collected in which chlorine, sulphur, and oxygen combine with several metals; the numbers representing the metals are kept constantly the same, for the greater facility of comparison.

Copper	60	+	32·77	chlorine	=	cuprane.
		+	67·20	ditto	=	cupranea.
		+	7·79	oxygen	=	orange oxide.
		+	15·00	ditto	=	brown oxide.
Tin	55	+	33·40	chlorine	=	stannane.
		+	67·00	ditto	=	stannanea.
		+	15·00	sulphur	=	gray sulphuret.
		+	31·00	ditto	=	aurum musivum.
		+	7·50	oxygen	=	protoxide.
		+	15·20	ditto	=	peroxide.
Iron	29·5	+	33·60	chlorine	=	ferrane.
		+	55·50	ditto	=	ferranea.
		+	8·00	oxygen	=	black oxide.
		+	13·20	ditto	=	red oxide.
Manganese	28·4	+	33·60	chlorine.		
Lead	97·2	+	33·80	chlorine	=	plumbane.
		+	15·09	sulphur	=	sulphuret.
		+	7·50	oxygen	=	yellow oxide.
Zinc	34·5	+	34·40	chlorine	=	zincane.
		+	7·50	oxygen	=	oxide.
Arsenic	21·9	+	33·60	chlorine	=	arsenicane.
		+	7·30	oxygen	=	white oxide.
Antimony	42·5	+	34·60	chlorine	=	antimonane.
		+	14·86	sulphur	=	sulphuret.
		+	7·50	oxygen	=	protoxide.
Bismuth	67·5	+	34·20	chlorine	=	bismuthane.
		+	15·08	sulphur	=	sulphuret.
		+	7·50	oxygen	=	oxide.

7. *On the Action of muriatic Acid on some Combinations of Chlorine and Metals.*

Sir Humphry Davy has pointed out in a great variety of instances, the existence of an analogy between chlorine and oxygen. He has shown that the former, united with certain inflammables, constitutes, like the latter, acid compounds; and combined with metals, as it has already been observed, substances similar in many respects to metallic oxides.

I have kept this analogy in view in my inquiries, and directed

directed by it in my experiments, I have obtained some results which appear to me to coincide with it.

Thus having been led to try the action of muriatic acid on different combinations of the metals and chlorine, I have found many of them capable of uniting with this acid, and of forming compounds not dissimilar to some of those consisting of acids and metallic oxides.

Corrosive sublimate, stannane, cuprane, and the combinations of chlorine with antimony, zinc, lead, and silver are all soluble in different degrees in muriatic acid.

Corrosive sublimate, which is but sparingly soluble in water, and still more sparingly in the sulphuric and nitric acids, is, I have ascertained, very readily soluble in muriatic acid. One cubic inch of the common strong acid takes up about 150 grains of this substance, and when gently heated, a quantity far more considerable, about 1000 grains. The compound thus formed solidifies on cooling into a crystalline fibrous mass of a pearly and brilliant lustre. It is decomposed by heat, the acid being first expelled, and when exposed to the atmosphere, it effloresces and appears to lose its acid, for afterwards analysed, it is found to be pure corrosive sublimate.

When I first tried the action of muriatic acid on the different combinations of chlorine already mentioned, I was not aware that Klaproth had before observed the solubility of horn silver in this acid, and Mr. Chenevix that of cuprane. Horn silver, cuprane, and horn lead are precipitated from muriatic acid, unaltered by water. Both the hot saturated solutions of the two last compounds deposit crystals on cooling; those from the solution of the former are of an olive green colour and of a prismatic form, and consist of cuprane and muriatic acid; those from the latter, are small white brilliant plates.

Finding the combinations of the metals and chlorine so generally soluble in liquid muriatic acid, I expected that some of them might absorb muriatic acid gas; but none that I have tried have possessed this property, not even the liquor of Libavius. Indeed this is not singular; for water is necessary to the composition of many saline bodies, neutral carbonat of ammonia and nitrat of ammonia, for instance, cannot be formed without the presence of water. Neither is the precipitation of cuprane, horn silver, and horn lead from muriatic acid by water extraordinary; there are several salts containing metallic oxides which are liable to the same change, the oxides having less affinity for the acid, than water has.

The

The action of muriatic acid on the combinations of the different metals and chlorine will, I have little doubt, afford, when more minutely investigated, explanations of many phænomena which are not yet well accounted for. Before I conclude, I shall mention only one instance to which it already appears to be applicable. M. Proust has observed the decomposition of calomel by boiling muriatic acid, and its conversion into corrosive sublimate and running mercury. Now calomel being insoluble in muriatic acid, these changes evidently appear to be owing to the strong attraction of the acid for corrosive sublimate, which has been already shown to exist.

XXXII. *A Correspondence between Dr.* Bostock *and Dr.* Marcet, *on the Subject of the uncombined Alkali in the Animal Fluids.*

To Mr. Tilloch.

Sir, The attention which I have for some time paid to the subject of animal chemistry, caused me to read with much interest the controversy, which was carried on through the medium of your Journal, between Dr. Pearson and Dr. Marcet, respecting the nature of the uncombined alkali in the serum of the blood. I was induced to make a considerable number of experiments upon the subject, the result of which had led me to decide in favour of Dr. Pearson's opinion; but having communicated my doubts to Dr. Marcet, he repeated and extended his former experiments in such a manner as, I think, firmly to establish the fact, that the alkali is soda. The detail of these experiments, as contained in the following letter of Dr. Marcet to me, I have his consent to transmit to you for publication, and I believe you will agree with me in the opinion that they must entirely set the question at rest.

I am, sir,

Your obedient servant,

Knot's Hole Bank, near Liverpool, Aug. 22, 1812. (Signed) J. Bostock.

Dr. Marcet *to Dr.* Bostock.

"London, August 19, 1812.

"My dear Friend,—I feel much indebted to you for the remarks you have made, and the doubts you have expressed in some of your last letters to me, respecting the nature of the uncombined alkali in the incinerated salts of serum;

serum; they have induced me to reconsider the question, and to add to my former inquiry on that head, a few new results, which, I flatter myself, will remove every shadow of doubt which may remain on your mind in that respect.

"In my reply to Dr. Pearson, in March last, I abstained purposely from bringing forward any new data, because the chief object of that letter was to vindicate former statements and inferences, and to show that there had not been, as was argued by my opponent, any blunder in the mode of reasoning by which I arrived at my conclusions. Indeed it appeared to me hardly necessary to push the inquiry any further; and I must own that from the manner in which Dr. Pearson had thought proper to carry on the controversy, in his two letters on the subject *, I should have felt great reluctance to resume the discussion, had it not been for your interference.

"Your objection, or rather your scepticism, arose from your having found in a mass of salts from serum (by the successive agency of acetic acid, alcohol, and tartaric acid,) such quantities of potash as appeared to you to show that the uncombined alkali was potash, and not soda; and you were further confirmed in this belief by observing that the alkaline residue obtained by heating the acetat to redness was deliquescent. You will see, however, by the following statements, that you were mistaken in your inference, and you will, I make no doubt, admit that the potash which you found in the alcoholic solution must have been in the state of muriat; and that the deliquescent quality of the alkaline residue must have arisen from your acetat having been but imperfectly decomposed, on account of the too low degree of ignition to which you had exposed it, and perhaps also (as you have yourself observed) in consequence of the presence of muriatic salts. But your experiments appear to show that the proportion which the muriat of potash in the blood bears to the muriat of soda, is greater than I had at first imagined; and that we had both underrated the power of alcohol to dissolve muriat of potash.

"As to the point at issue, however; namely, the nature of the uncombined alkali in the incinerated salts of blood, the experiments upon which I think myself warranted to repeat, with increased confidence, my former opinion, that the alkali is soda, and not potash, were conducted in the following manner.

* See this Journal for February and May last.

"After evaporating some human serum to siccity, incinerating the residue, dissolving in water the soluble saline substances contained in the incinerated mass, filtering this solution and evaporating it again, the alkaline mass of salts thus obtained was treated with acetic acid, and afterwards digested with five or six times its weight of alcohol of the specific gravity of 815. The highly deliquescent residue deposited by the evaporation of the filtered alcoholic solution was then made red hot in a platina crucible, and kept for a few minutes in a state of igneous fusion. A carbonaceous alkaline mass remained in the crucible, which, after being exposed to the air 48 hours, in a room without fire, and in damp though warm weather, did not exhibit the least vestige of deliquescence. This mass, the quantity of which amounted to four or five grains, being dissolved in a little water, was divided into four portions, *a*, *b*, *c*, *d*.

"The portion *a*, being examined by re-agents, exhibited the following properties:

"1. It contained abundance of muriatic acid.

"2. When suffered to evaporate spontaneously in a glass capsule, it left at the end of twelve hours a dry efflorescent crystalline substance which consisted principally of feathery crystals, amongst which were discovered groups of rectangular plates and a few minute cubes.

"3. The presence of potash in this crystalline mass was made obvious both by the tartaric acid, and by oxymuriat of platina, though not so much so by the latter of these tests.

"The portion *b* was saturated with sulphuric acid, and submitted to spontaneous evaporation. The result was a rim of confused crystals, surrounding a group of regular efflorescent prisms of Glauber, being (at least some of them) terminated by distinct dihedral summits, and having sufficient magnitude to be identified by the naked eye, even at the distance of a few yards; they were made to crystallize over and over again, always with the same result; but in some of these crystallizations a few crystals of sulphat of potash also appeared, the form of which was not equivocal.

"The portion *c*, being treated with nitric acid, yielded by evaporation great numbers of rhomboidal crystals, perfectly distinct to the naked eye, and amongst which no form at all resembling that of nitre could be detected.

"The portion *d* being treated with oxymuriat of platina,

tina, the usual crystalline appearance of potash-muriat of platina took place immediately; but by slow spontaneous evaporation, other and more abundant needle shaped crystals of soda-muriat of platina made their appearance.

"My conclusion, therefore, (which I hope will now also be yours,) is precisely as before; namely, that the potash which exists in the animal fluids is in the state of muriat, and that the whole of the uncombined alkali is soda; and as it is a known fact that muriat of potash is in some degree soluble in alcohol, the circumstance which led you into error is easily explained.

"I have only further to add, that the fact which I have endeavoured to establish by a specific inquiry, ought to have been inferred from principle; for it is well known that potash has a stronger attraction for the muriatic acid than soda; and indeed I understand that it is a common process in some manufactures, to obtain soda by the action of potash-ley on muriat of soda.

"Believe me ever, &c. &c.

"ALEX. MARCET.

"P.S.—Since the above was written, I have, in consequence of your suggestion, 'that the blood of graminivorous animals might perhaps yield potash instead of soda, on account of their living exclusively upon vegetable food,' examined bullock's blood with a view to ascertain this circumstance; and as there was no difficulty in procuring any quantity of that blood, I had some gallons evaporated, from which I procured some ounces of salts, in order to satisfy those who think that nothing certain can be inferred from experiments upon a small scale. However, the results were precisely similar, except that the crystals of sulphat and nitrat of soda obtained by the processes above detailed, were of much larger dimensions than in any of my former experiments."

XXXIII. *Dr.* ROGET *in Reply to Mr.* HUME *on his Test for Arsenic.*

To Mr. Tilloch.

SIR, IN the last number of your Magazine, Mr. Hume has thought proper to repeat a complaint which he had made in the Medical and Physical Journal for August last, of my not having paid sufficient attention, in the paper to which he alludes*, to a method which he had proposed some

* "A Case of Recovery from the Effects of Arsenic," published in the second volume of the Medico-Chirurgical Transactions.

some years ago, for the detection of minute quantities of arsenic. Lest any of your readers should be misled by the assertions of Mr. Hume, I beg leave to refer them to my answer to his letter, which has appeared in the last number of that Journal. It will there be seen, that so far is the charge of plagiarism from being well founded, that, in the statement of which he complains, I have not only quoted Mr. Hume's name, but have specifically pointed out his claims to originality, and referred distinctly, not only to the document on which they rest, but also to a work in which a full account of all the modifications of his process, which he afterwards published, is contained. To those who will take the trouble of reading what Mr. Hume had written on the subject previous to the appearance of my own paper, it will be sufficiently evident that the mode proposed by Dr. Marcet is essentially different from any of those mentioned by Mr. Hume, who, in recommending the employment of nitrate of silver, has not even hinted at the possibility of substituting ammonia for the fixed alkalies. In my answer to his letter I stated several reasons which led me to prefer the former. This would now be a superfluous task, since Mr. Hume has himself become its panegyrist; for in speaking of a test similar to Dr. Marcet's, and which he calls *the ammoniaco-nitrate of silver*, he pronounces that "it must now supersede all other tests for arsenic, and become the standard to future operators."

When a test similar to the one on which Mr. Hume has bestowed this high eulogium first occurred to Dr. Marcet, and when we ascertained together all the collateral circumstances mentioned in my paper respecting the limits of its power, and the agencies of other metallic bodies on the same test, we had not the smallest knowledge or recollection of Mr. Hume's ever having turned his attention to the subject of arsenic; nor did any of the chemical friends, to whom our experiments were shown, appear to have noticed what he had written on the subject. It was only at the moment of sending our remarks to the press, that Dr. Marcet met with a quotation in Dr. Henry's Elements of Chemistry, which led me to read the letters of Mr. Hume, to whose claims I was anxious to do, as I trust I have done, every possible justice in the notice that I inserted in my paper.

I am, sir,
Your obedient servant,
P. M. ROGET.

Bernard Street, Russel Square,
Sept. 7, 1812.

XXXIV. *Che-*

XXXIV. *Chemical Researches on the Blood, and some other Animal Fluids.* By William Thomas Brande, *Esq. F.R.S. Communicated to the Society for the Improvement of Animal Chemistry, and by them to the Royal Society.*

[Concluded from p. 117.]

SECTION VI.

Researches on the colouring Matter of the Blood.

1. To procure this substance for experiments, I generally employed venous blood which had been stirred during its coagulation; the fibrina is thus removed, and the colouring matter diffused through the serum, from which it gradually subsides, being difficultly soluble in that fluid; on decanting off the supernatant serum, the colouring matter remains in a very concentrated form. When other modes of procuring it were employed they will be particularly mentioned; but as I have not found the serum which is retained interfere much with the effects of various agents upon the colouring principle, the method just noticed was commonly adopted.

2. When the colouring matter thus collected is microscopically examined, it seems, as Lewenhoeck first observed*, to consist of minute globules. These are usually described as soluble in water, a circumstance which my own observations led me to doubt, and which the more accurate experiments of Dr. Young, an account of which, intended for publication, he has kindly permitted me to peruse, have completely disproved.

3. The effect of water upon the red globules, is to dissolve their colouring matter, the globule itself remaining colourless, and, according to Dr. Young, floating upon the surface.

This aqueous solution is of a bright red colour, and not very prone to putrefaction. When heated, it remains unaltered at temperatures below 190° or 200° Fahrenheit; at higher temperatures it becomes turbid, and deposits a pale brown sediment: if in this state it be poured upon a filter, the water passes through without colour, so that exposure to heat not only destroys the red tint, but renders the colouring matter insoluble in water.

Alcohol and sulphuric ether added to this solution also render it turbid, and when these mixtures were filtrated, a colourless and transparent liquor was obtained.

* Haller Elem. Physiolog. vol. i. p. 51.

 4. The

4. The matter remaining upon the filter was insoluble in water, in alcohol, and in sulphuric ether; but when digested in dilute muriatic or sulphuric acid, a portion was taken up forming a brown solution. I regard this soluble portion as a modification of the colouring matter produced by the operation of heat: the insoluble residuum had the properties of albumen.

5. *Effects of Acids on the colouring Matter.*

A. Muriatic acid poured upon the colouring matter of the blood, renders one portion of it nearly insoluble and of a bright brown colour: another portion is taken up by the acid forming a dark crimson solution when viewed by reflected light; but when examined by transmitted light, it has a greenish hue.

This solution remains transparent, and its colour is unimpaired by long exposure to light, either in contact with the air, or when kept in close vessels. At its boiling temperature the colour is also permanent.

Infusion of galls produces no change in this muriatic solution, nor is its colour affected by carbonated alkalies, even when added in considerable excess.

It is rendered brown red by supersaturation with caustic potash, but not with soda, nor ammonia: these, and especially the latter, rather heighten its colour.

When considerably diluted with water its original colour is much impaired, and the green hue, which it always exhibits by transmitted light, becomes more evident.

In preparing this solution, I frequently employed the coagulum of blood cut into pieces, and digested in equal parts of muriatic acid and water, at a temperature between 150° and 200°. In three or four hours the acid was poured off, and filtrated. The clear solution was in all respects similar to that above described, although before filtration it appears of a dirty brown colour.

I evaporated a portion of this muriatic solution in a water-bath, to dryness; it retained its colour to the last, and left a transparent pellicle upon the evaporating bason, of a dirty red colour: this when redissolved in muriatic acid acquired its former tint, but the colour of its aqueous solution was nearer brown than red.

B. Sulphuric acid, diluted with eight or ten parts of water, forms an excellent solvent of the colouring principle of the blood.

It

It may be employed in a more concentrated state; but the bright colour of the solution is in that case apt to be impaired, and when more largely diluted with water, its action is slow and uncertain. Either the sediment of the colouring matter from the serum, or the crassamentum of the blood, may be indifferently employed in forming these solutions.

When dilute sulphuric acid is added to the colouring matter, it renders it slightly purple; and if no heat be applied, the acid when poured off and filtered, is colourless; so that dilute sulphuric acid when cold, does not dissolve this colouring principle.

One part of the crassamentum of blood cut into pieces, was put into a matrass placed in a sand heat, with about three parts of dilute sulphuric acid. It was kept for twelve hours in a temperature never exceeding 212°, nor below 100°. After twenty-four hours the acid was filtered off, and it exhibited a beautiful bright lilac colour, not very intense, and teinted with green when viewed by transmitted light.

This solution is nearly as permanent as that in the muriatic acid. Some of it which has been kept for a month in an open vessel, often exposed to the direct rays of the sun, is very little altered.

When diluted with two or three times its bulk of water, the lilac tint disappears, and the mixture is only slightly green.

When exposed to heat, the colour gradually changes as the acid becomes more concentrated by evaporation, and when reduced to about half its bulk the lilac hue is destroyed.

The solutions of pure and carbonated alkalies when added in excess, convert the colour of this sulphuric solution to brownish red; but in smaller quantities, they merely impair it by dilution.

C. Nitric acid, even much diluted, is inimical to the colouring matter of the blood.

A few drops added to the muriatic or sulphuric solutions gradually convert their colour to a bright brown, and larger quantities produce the same change immediately.

The action which this acid exerts upon the colouring matter under other circumstances is nearly similar, and always attended with its decomposition, so that my attempts to procure a red solution in this menstruum uniformly failed of success.

D. Acetic acid dissolves a considerable quantity of the

 colouring

colouring matter of the blood; the solution is of a deep cherry red colour. When somewhat diluted, or when observed in tubes of about a quarter of an inch bore, this solution appears perfectly green by transmitted light. In its other habitudes it nearly resembles the muriatic solution.

E. The solution of the colouring matter in oxalic acid is of a brighter red than those hitherto noticed; that in citric acid is very similar to the acetic solution, and with tartaric acid the compound somewhat inclines to scarlet. All these solutions exhibit the green hue, to which I have so often alluded, in a remarkable degree.

6. *Effects of Alkalies on the colouring Principle of the Blood.*

The caustic and the carbonated alkalies form deep red solutions of this substance, which are extremely permanent.

1. Solutions of pure potash, and of the subcarbonate, take up a large proportion of the colouring matter of the blood. The intensity of the colour of this solution, when concentrated, is such, that it appears opake, unless viewed in small masses, or in a diluted state, when it is of a bright red colour.

2. In soda and its subcarbonate, the solution has more of a crimson hue, which colour is extremely bright in its concentrated state.

3. The solution in liquid ammonia approaches nearer to scarlet than that in which the fixed alkalies are employed.

4. When these alkaline solutions are supersaturated with muriatic acid, or with dilute sulphuric acid, they acquire a colour nearly similar to the original solutions in those acids which have been above described.

5. Nitric acid added in small quantities, or even to saturation of the alkaline menstruum, heightens the colour of the three compounds; but when there is a slight excess, a tint of orange is produced, which soon passes into bright yellow.

6. The alkaline solutions may be evaporated nearly to dryness without losing their red colours; during the evaporation of the ammoniacal solution, the alkali flies off, and a brown-red solution of the colouring matter in water remains.

Having ascertained the above facts respecting the colouring principle of the blood, I next proceeded to examine how far it was susceptible of entering into those combinations

tions which are peculiar to other varieties of colouring matter.

These experiments I shall detail in the order in which they were made.

1. Some pure alumine was added to a concentrated aqueous solution of the colouring matter of the blood, and after twenty-four hours the mixture, which had been frequently agitated during that period, was poured upon a filter, and the residuum washed with hot distilled water.

The filtrated liquor had lost much of its original colour; the alumine had acquired a red tinge; it was dried at a temperature between 70° and 80°, during which it became brown.

2. Two hundred grains of alum were dissolved in four fluid ounces of a solution of the colouring matter, similar to that employed in the last experiment. The colour of the compound was bright red. Liquid ammonia was added, and the precipitate collected, and carefully dried. It was of a dirty red, and after some days exposure to light became nearly brown.

From these, and other experiments which I have not thought it necessary to detail, it appears that alumine will not form a permanent red compound with the colouring principle of the blood; I was therefore next induced to employ oxide of tin.

3. Fifty grains of crystallized muriate of tin (prepared by boiling tin filings in muriatic acid, and evaporating the solution.) were dissolved in four ounces of the solution of colouring matter, which immediately assumed a purple tint, and became afterwards brown. It was diluted with twice its bulk of water, and put aside in a stopped phial. On examining it three days afterwards, a small quantity of a bright red powder was observed at the bottom of the phial, which proved to consist of the colouring principle combined with the metallic oxide. A portion of this compound which has been kept in water for some weeks has undergone no change of colour; but when dried by exposure to air, it loses its brilliant tint, and becomes of a dull red hue.

To a compound solution of muriate of tin and colouring matter, similar to that employed in the last experiment, I added a sufficient quantity of solution of potash to decompose the salt of tin. The precipitate thus obtained was collected, and dried by exposure to the air of a warm room. It was of a dull red colour, and has undergone no apparent change

change by exposure to the joint action of light and air for three weeks.

4. Finding that supertartrate of potash exalted the colour of the blood, I endeavoured to form a compound of it with that substance and oxide of tin, and thus, in some measure, to imitate the process in which cochineal is employed for the production of scarlet dye; but although a bright red compound is produced, when it is dried at a very moderate temperature its colour becomes similar to that of the other combinations which I have described.

These experiments I repeated in various ways, occasionally applying the salt of tin as a mordant to woollen cloth, linen, &c.; but the brilliancy of the colour was never permanent.

5. Having observed that infusion of galls and decoction of oak bark do not impair the colour of the blood, I conceived that solution of tannin might answer the purpose of a mordant, as it is effectually employed by dyers in giving permanence to some of their red colours.

I accordingly impregnated a piece of calico with decoction of oak bark, and afterwards passed it through an aqueous solution of the colouring matter of blood. When dry, it was of a dirty red colour, nearly similar to that which would have been obtained had no mordant been applied: when however an alkaline solution of the colouring matter was employed, the colour was equal to that of a common madder red, and as far as I have been able to ascertain, it is permanent.

6. A solution of superacetite of lead was impregnated with the colouring matter of the blood. The compound was bright red: no spontaneous change took place in it, and on the addition of an alkali a white precipitate was formed, the fluid retaining its former tint.

From this and other experiments, in which it was attempted to combine oxide of lead with the colouring of the blood, it would appear that there is no attraction between those two substances.

7. The most effectual mordants, which I have discovered for this colouring matter, are some of the solutions of mercury, especially the nitrate, and corrosive sublimate.

Ten grains of nitrate of mercury (prepared with heat and containing the red oxide) were dissolved in two fluid ounces of a solution of the colouring of the blood. After some hours a deep red compound was deposited, consisting chiefly of the metallic oxide combined with the colouring matter,

matter, and a small portion of coagulated albumen. The remaining fluid had nearly lost its red colour.

The nitrate of mercury containing the black oxide, produces nearly similar effects, excepting that the colour of the compound is of a lighter red.

When corrosive sublimate is added to the solution of the colouring matter, its tint is instantaneously brightened, and it becomes slightly turbid from the deposition of albumen. If this be immediately separated by a filter, the liquor which passes through gradually deposits a deep red or purplish insoluble precipitate, and if it now be again filtrated the liquid is colourless, the whole of the colouring principle being retained in the compound which remains upon the filter.

By impregnating some pieces of woollen cloth with solution of nitrate of mercury, or of corrosive sublimate, and afterwards steeping them in an aqueous solution of the colouring matter of the blood, I succeeded in giving them a permanent red tinge, unalterable by washing with soap; and by employing the ammoniacal solution of the colouring matter, calico and linen may be dyed with the same mordant.

In these experiments I was much satisfied by the complete separation of the colouring matter from its solutions, which after the process were perfectly colourless.

SECTION VII.

Some Remarks on the preceding experimental Details.

From the experiments related in the second section of this paper, it appears that sulphuric acid effects changes upon the coagulum of chyle, similar to those which Mr. Hatchett has observed to result from the action of dilute nitric acid upon the coagulated white of egg. This last substance, however, is not convertible into gelatine by means of sulphuric acid, whereas in these respects the curd of milk resembles that of chyle: this circumstance, as well as the more ready solubility of the coagulum of chyle in dilute than in concentrated acids, points out a strong analogy between those two bodies

The sweet taste of chyle naturally suggested the idea of its containing sugar *; but I am not aware of any direct experiments which have demonstrated its existence, and have therefore detailed minutely such researches as I have been enabled to make upon the subject, hoping at some future period to render them more complete.

* Fordyce on Digestion, 2d edition, p. 121.

The

The experiments to prove the non-existence of gelatine in the serum of blood, will, I trust, be deemed sufficiently decisive: they show that that abundant proximate principle of animals is not merely separated from the blood, in which it has been supposed to exist ready formed, but that it is an actual product of secretion.

The proportion of iron afforded by the incineration of several varieties of animal coal, is much less considerable than we have been led to expect; and the experiments noticed in the fifth section show that it is not more abundant in the colouring matter of the blood than in the other substances which were submitted to examination; and that traces of it may be discovered in the chyle which is white, in the serum, and in the washed crassamentum or pure fibrina.

The inferences to which I have alluded, in the first section of this paper, are strongly sanctioned by these facts, and coincide with the opinion which has been laid before the Royal Society, by Dr. Wells*, respecting the *peculiar nature* of the colouring principle of the blood, and support the arguments which are there adduced.

That the colouring matter of the blood is perfectly independent of iron, is, I conceive, sufficiently evident from its general chemical habitudes, and it appears probable that it may prove more useful in the art of dyeing than has hitherto been imagined, since neither the alkalies nor the acids (with the exception of the nitric) have much tendency to alter its hue. The readiness, too, with which its stains are removed from substances to which no mordant has been applied, seem to render it peculiarly fit for the purposes of the calico-printer. I have not extended these experiments, nor have I had them repeated on a sufficient scale to enable me to draw more general conclusions respecting the possibility of applying them with advantage in the arts: this would have led me into too wide a field, and one not immediately connected with the objects of this Society: the subject, however, appears important.

It is not a little remarkable that blood is used by the Armenian dyers, together with madder, in the preparations of their finest and most durable reds †, and that it has even been found a necessary addition to insure the permanency of the colour ‡. This fact alone may be regarded as demonstrating

* Phil. Trans. 1797.

† Tooke's Russian Empire, vol. iii. p. 497.

‡ Aikin's Dictionary, art. *Dyeing*, and Philosoph. Magazine, vol. xviii.

monstrating the non-existence of iron as the colouring principle of the blood, for the compounds of that metal convert the red madder to gray and black.

Whilse engaged in examining the colouring matter of the blood, I received from Mr. William Money, house surgeon to the general hospital at Northampton, some menstruous discharge, collected from a woman with prolapsus uteri, and consequently perfectly free from admixture of other secretions. It had the properties of a very concentrated solution of the colouring matter of the blood in a diluted serum, and afforded an excellent opportunity of corroborating the facts respecting this principle, which have been detailed in the preceding pages. Although I could detect no traces of iron, by the usual modes of analysis, minute portions of that metal may and probably do exist in it, as well as in the other animal fluids which I have examined; but the abundance of colouring matter in this secretion should have afforded a proportional quantity of iron, did any connection exist between them. It has been observed that the artificial solutions of the colouring matter of the blood invariably exhibit a green tint when viewed by transmitted light: this peculiarity is remarkably distinct in the menstruous discharge *.

I hope that some of the facts furnished by the above experiments may prove useful to the physiological inquirer: they account for the rapid reproduction of perfect blood after very copious bleedings, which is quite inexplicable upon that hypothesis which regards iron as the colouring matter, and may perhaps lead to the solution of some hitherto unexplained phænomena connected with the function of respiration. There can, I think, be little doubt that the formation of the colouring matter of the blood is connected with the removal of a portion of carbon and hydrogen from that fluid, and that its various tints are dependent upon such modifications of animal matter, and not, as some have assumed, upon the different states of oxidizement of the iron which it has been supposed to contain.

* I could discover no globules in this fluid; and although a very slight degree of putrefaction had commenced in it, yet the globules observed in the blood would not have been destroyed by so trifling a change.

XXXV. *On*

XXXV. *On Sir* Humphry Davy's *late Proposal for improving the Arts of bleaching Linen and Cotton Cloth by substituting the Oxymuriate of Magnesia for the Oxymuriate of Lime in the bleaching Process; with some Observations in Reply to an Article in the Philosophical Magazine for June. By* James Ogilby, *M. D.*

To Mr. Tilloch.

Sir, I have noticed in your valuable publication for last June, some observations upon a communication which I had the honour of laying before the Kirwanian Society, relative to a proposal which Sir H. Davy suggested for the improvement of the process of bleaching linen and cotton cloth in Ireland, in the course of a lecture delivered by him at the Dublin Society, upon the subject of oxymuriatic acid and its combinations. The person who has signed his name to those observations has thought proper to apply some very strong expressions to my communication; he terms it an "incorrect statement," accuses me of "ignorance of the facts," brings forward a letter from his brother, a Dublin apothecary, in further confirmation of the "inaccuracy of my statements," and seems to feel a degree of satisfaction, upon the dreadful fate they will meet with in the scientific world, according to his expectation.

Should I succeed, in the following pages, in satisfying all those of your readers who are interested in and competent judges of this subject, that those observations upon my paper contain an erroneous, partial and mutilated statement of the facts, that those persons who have thought proper to stand forward as the advocates for Sir Humphry's proposal are totally destitute of all knowledge of the subject, and have throughout the discussion betrayed the grossest ignorance upon every topic connected with it, and that my original statement stands perfectly correct; I hope that any expressions I may use will not be ascribed to a desire on my part to imitate the example of my assailant, but to the difficulty which naturally presents itself to the rebutting of misrepresentations, without employing terms which to some may appear harsh. As to the irrelevant epithets employed by the person alluded to, who I understand is a young Irish operator in the laboratory of the Royal Institution, having nothing to do with the point at issue, it is not necessary that I should take any further notice of them.

It has been suggested to me, that as my observations were

were levelled at the proposal of Sir H. Davy, and the animadversions upon them have issued from the laboratory of the Royal Institution, a department over which he has the entire control, I should be justified, under such circumstances, in considering him as the author, and the person who signs his name as no more than a mere in-trument, and that I should consequently leave the latter altogether unnoticed in the controversy. It would, however, be unfair to hold Sir Humphry accountable for the indiscreet zeal of his admirers. It is perfectly obvious that the article inserted in your number for June could not have been written by that gentleman, and it is possible that the animadversions alluded to were published even without his knowledge: but as they must since have met his eye, as they were intended to serve him, and as they may be considered as flowing from his own proposition, it is impossible I should keep him out of view in the observations I have now to offer. It would however perhaps be expecting too much to look for an answer from himself, considering how many important objects must occupy his attention.

I shall now proceed to put your readers in possession of as clear and succinct an account of the facts and circumstances connected with Sir Humphry's proposal, as full and accurate notes of his lectures, and considerable attention to the subject enable me to do; and shall add, as I go along, the substance of my former remarks, as well as some observations in reply to the article in your publication for June, already adverted to.

In the years 1810 and 1811, Professor Davy, at the request of the Dublin Society, delivered two very short courses of lectures, which he termed *Electro-chemical*, in the laboratory attached to that Institution. In the course of those lectures, when treating of oxymuriatic acid and its alkaline and earthy combinations, he necessarily alluded to the subject of bleaching, and stated how happy he felt himself, that upon a subject of such national importance he had it in his power, in return for the great politeness and attention with which he had been received and heard in Ireland, to offer a proposal which he conceived would materially benefit that art. He then observed, in speaking of the oxymuriate of lime, now generally used in the process, that this salt he conceived could not, with safety to the cloth, be used in bleaching; for he observed, he said, that bits of linen cloth steeped in a *strong* solution of *muriate of lime* were much injured in their texture, or actually rotted.

rotted. These experiments were however not detailed, or any satisfactory account given of them, nor were they ever committed to print. "Now," said he, in proceeding, "if a *strong* solution of muriate of lime destroys the vegetable fibre, a *weak* one must have a similar, though a diminished effect; and therefore the *oxymuriate of lime* cannot be used with safety in bleaching." This was the only reason advanced by the learned Professor in urging the propriety of adopting his important proposal, and rejecting an article which has been for years in general use for whitening even the most delicate fabrics of flax and cotton in every part of the empire, not to say of Europe. In support of this reasoning, the Professor related a story of a respectable apothecary in Dublin, who in the way of business was preparing some muriate of lime for a customer, and that in drying the crystals upon a cotton cloth, it was found quite rotten after the operation. Many doubts as to this supposed fact immediately arose in the minds of some of the hearers; some suggested that the cloth might have been previously purged by active preparations, or caustic alkalis; others, that the liquor of the crystals might have contained free muriatic acid; or, what is not very improbable, that the cloth might have come rotten from the hands of the manufacturer.

It would be needless to multiply arguments, to show the impropriety which there would be in admitting such an observation as this in support of a proposal to introduce a material alteration or improvement in an art of such vital consequence to Ireland, and so intimately connected with her staple manufacture.

This advocate of Professor Davy takes upon him to deny his ever having made use of the incorrect mode of reasoning which I have attributed to him, and which I have quoted from notes taken at his lectures, and attempts to obviate the force of my observations by substituting a sentence, which I can prove was used, not in treating of the subject in question, but upon an occasion altogether different.

In taking notes of Sir Humphry's lectures, I never attempted, nor perhaps was it possible, to take down the language *verbatim;* but that the expressions I have attributed to him contain the substance of what he said, and what he meant to convey, and nothing else; and that the sentence could not be altered in its meaning by any mere change of diction, I must positively maintain, and will continue to do so, as I have the strongest confirmation of it it, from the circumstances I shall immediately relate, and from several well informed persons who attended the lectures and compared their notes with mine. Conversations upon the doctrine, "that if a strong solution proves detrimental a weak one must do so proportionally," took place immediately after the lecture, so as to impress it strongly upon the memories of those concerned; and I may mention an incidental circumstance confirming fully the accuracy of my statement, and which occurred during a conversation that took place upon this subject at the last meeting of the Kirwanian Society. A gentleman remarkable for the tenaciousness of his memory, who attended Sir Humphry's last course of lectures in Dublin, and who was particularly attentive in collecting the general principles of the science laid down by the lecturer, no sooner heard what the point in dispute was, than he had a perfect recollection of it, and accounted for the strong impression it had made upon his mind, by remarking, that for some years before he had been in the habit of using, by regular advice, very dilute sulphuric acid as a tonic for his stomach; and it immediately struck him, upon hearing the new doctrine, that this practice must have been extremely imprudent, since it is undeniable that undiluted sulphuric acid will instantly destroy that important organ, and life itself.

It is now said that the argument which the Professor advanced in recommending his new project was not as I have stated, but was as follows: "that bodies in their nascent state acted with more energy than when they had been formed." It is very true that Sir Humphry did use these words, so familiar to every student in the science; but it was not in speaking of the *oxymuriate* of magnesia that he used them, nor can they have any thing to do with it, as to its superiority in bleaching over other compounds. It was in speaking of the action of the oxygen of the water upon the carbonaceous principle of the colouring matter, before it perfectly assumes the gaseous form.

It is really surprising how the person who signs his name to a virulent attack upon my statement could gravely thrust himself forward as an evidence upon this occasion; and, with an affectation of speaking sarcastically of modesty, adduce a quotation as if it were written by himself at the time of the lecture, and this in opposition to a number of gentlemen who were previously conversant with the principles of the science, and who were exclusively employed in noting down every experiment and observation of of the lecturer Is it possible to suppose that an individual, who was busily and usefully employed during the lectures in attending the experimenter, scouring the Voltaic wires, dipping the plates, charging retorts, blowing the fire, or working the air-pump, can be brought forward with the smallest effect against many of Sir Humphry's audience, of long standing in the science, and whose attention was not diverted from the immediate object of the lecture by similar duties? It is rather fortunate for me however, in this controversy, that could I even grant that I was in error upon this point, and relinquish my ground, my objections to the proposed new process would still remain in full force, and the more so, since the ground has been shifted upon which it originally rested.

It can now be no longer maintained by any one, that this proposal should be adopted by the bleacher, *because a strong solution of muriate of lime rots linen cloth* (supposing it an established fact), the conclusion drawn from this being now given up, *that a weak solution should do so likewise.*

But let us examine if the chemical law already quoted (" that bodies in their nascent state act with more energy than when they have been formed") can possibly be brought forward in support of this project, or if it has the smallest bearing upon the subject. Oxymuriate of lime is, I presume, according to the Professor's view, a triple compound of oxygen, oxymuriatic acid, and *calcium;* and no sooner is this combination dissolved in water and brought into contact with unbleached linen, than it is instantly disturbed; the oxygen of the salt, along with a portion of oxygen from the water, (which the oxymuriatic acid is said to decompose,) acts upon the colouring matter of the cloth, and of course bleaches it. Now, supposing for a moment this *hypothesis* (*theory* according to Mr. John Davy) were the true one, it has evidently nothing to do with the proposal of applying the magnesian salt in place of any other. But it is not difficult to foresee, in this case, a " straw which may be caught at by a person drowning." Its adherents may say, " Grant us that a strong solution of muriate of lime destroys the vegetable fibre, and then we will give you muriate of lime in its *nascent state* in contact with the linen in the steeping process; and though it is surrounded with millions of particles of water to which it has a very powerful affinity,—yet it must destroy all before it, and that too at the very moment that the oxygen is acting upon the vegetable colour upon the surface of the cloth." I cannot conjecture

conjecture any other than this untenable speculation, which can be brought forward to prop up this favourite proposal, of such "*national consequence to Ireland.*"

But will a set of changes rung upon technical terms convince the practical bleacher, that he ought to discard a substance which he has used for years in whitening the most delicate fabrics, with perfect and proved safety, and to substitute another compound, one of the ingredients of which he has no known means of procuring at less than *one hundred* times the expense of the other? The best magnesia in Ireland cannot be purchased for less than 2*s.* 2*d.* per lb.; the worst, 1*s.* 8*d.* Lime is not *one farthing* per pound.

It is not possible that Sir Humphry can be ignorant that it is a primary object with the bleacher, not only to send his goods to market perfectly sound, but to use such an *œconomical* process as shall enable him to meet his competitors in the market upon equal terms, and adjust his prices to the demand.

No matter then how apparently eligible any new ingredient proposed may be, or how applicable to his art; he is necessitated to decline its use if very expensive, unless he finds it indispensable to his process.

In treating of the *oxymuriate of potash,* an article which is yet used in bleaching, and to some extent in this country, the only objection which Sir Humphry stated (for *oxymuriate of potash* has no action on the vegetable fibre) against its use in the general business of bleaching, was its greater expense than *oxymuriate of lime.* This objection applies with four-fold force against the use of oxymuriate of magnesia, which would cost four times the price of oxymuriate of potash, and at least six times the expense of oxymuriate of lime! A manufacturer of bleaching salts in this city assures me that the salt of *magnesia* could not be supplied to the bleacher for less than 4*s.* per lb. The salt of lime is only 8*d.*! And it is a fact worth mentioning, for the information of Sir Humphry, that many bleachers in Ireland are restrained from even the use of the *oxymuriate of lime,* for no other reason than its high price; so that, when the weather is favourable for whitening, they depend entirely upon the old process.

It is not *mere* processes (for we abound with them), that we want, to accomplish almost every desideratum in the arts, but it is *œconomical* processes, which shall be consistent with and applicable to practice. If Sir Humphry, or any one else, will furnish the Irish bleachers with magnesia at *one penny* per lb., his proposal will receive attention. But considering my arguments against its adoption as well founded, I have a right to maintain, that I was justifiable in using strong, though by no means personal language, for the purpose of "deterring the *manufacturer* [bleacher] from acting upon it, by showing that it was not only unnecessary, but absurd and ruinous for him to attend to it. I understand that the Professor had an interview with the trustees of the linen manufacture,—but I have never heard that they have taken any notice of his new proposal; nor have conceived it necessary to issue any new precautionary directions to the bleachers against the use of the oxymuriate of lime.

I am accused of having erroneously stated as an assertion of Sir Humphry's, that the salt of magnesia had *entirely* superseded the salt of lime among the bleachers in Ireland. I have to remark that this defender of Sir Humphry has thought proper to slip in the word "*entirely*," on his own authority, to give perhaps the assertion a little more force in *my favour*. But it is worth the reader's while to remark what is the answer to this. It is, that Mr. Davy has only asserted that an enlightened *individual* has yet acted upon his proposal. Thus, then, it is brought forward as a matter of triumph, that almost two years after its birth, a single individual, not a bleacher but a calico-printer, has applied to practice a proposal the professed object of which was to benefit the staple manufacture of Ireland.

To show, however, that I was not altogether unwarranted in making the assertion, that Sir Humphry had adverted to more than an *individual* having embraced his proposal, I beg leave to introduce the following passage from the Philosophical Magazine for March, p. 225, and from an abstract of a lecture delivered by Sir Humphry at the Royal Institution. It is in his 8th lecture: "A compound of *chlorine* and lime is commonly employed in *bleaching*, but even this *injures the fabric of the linen*. Mr. Davy has found that a compound of chlorine and magnesia bleaches *without affecting the vegetable fibres*, and on his suggestion it has been used with success by *some manufacturers* in Ireland." Mr. Duffy, a gentleman engaged in a very extensive calico-printing concern near Dublin, whose respectability and scientific character from a long acquaintance are well known to me, is brought forward as an evidence in favour of the new proposal. I do not hesitate, however, to state that, upon the subject of Mr. Duffy's opinion, there is a mistake. I thought it my duty to call upon

upon Mr. Duffy, when I found my statements so coarsely attacked; and he assured me, and gave me authority to state, that in the process of what is properly called *bleaching*, he had never given his approbation of the employment of the oxymuriate of magnesia; it was only in a particular operation in calico-printing, called *clearing*, that he could see any reason for using this substance; and it was not because *muriate of lime* destroyed the vegetable fibre that he was led to the use of it, but upon different principles, not one of which the Professor even hinted at in any of his lectures before Mr. Duffy had sent a communication to him upon the subject. It is also with the full permission of Mr. Duffy, that I state it as his opinion, that the *oxymuriate of lime* can be employed in *bleaching* with perfect safety; and the fact is, that in his preparatory process of whitening the cottons he still uses this salt. He says, indeed, that if any one will give him magnesia at *one penny* per pound, he will then prefer the magnesian salt; but not at all in consequence of Sir Humphry's opinion that the salt of lime is injurious to the texture of the cloth, but for other reasons, and principally because magnesia has much less action upon the *dyed* colours than lime has;—a subject upon which the Professor did not give the slightest hint at the time he made his proposal. If Mr. Duffy, in a polite letter to Sir Humphry, gave him credit as the suggester of such an improvement, it can only be inferred that Mr. Duffy has been extremely liberal, and has acted with much self-denial; for I am fully convinced that he himself has the whole credit of the improvement; for unless mentioning the mere name of a substance can be construed into a hint to the calico-printer to apply it to a particular process, Sir Humphry has no claim whatsoever to be considered as the proposer*.

That Mr. Duffy has not at any time approved of the rejection of oxymuriate of lime by the *bleacher*, the following passage in his letter to Sir Humphry evinces: "The introduction of the oxymuriate of lime by Mr. Tennant

* "In searching for a substance which possesses none of those pernicious qualities, (alluding to the action of lime upon the *dyes*,) we have found that the oxymuriate of magnesia in every respect answers in the most complete manner, not only for *clearing* the white ground of the goods, but also in preserving the colours of the same shade which they were originally.

"Of all the earths which are partially soluble in water, magnesia possesses the property of changing colours least, the alteration made by it upon paper stained with litmus being scarcely perceptible. It is therefore peculiarly fitted, when united with oxygen, for the purpose of clearing the stain from the white of printed goods." *Edinburgh Cyclopædia*, article *Bleaching*.

 has

has left the mere *bleacher* of cotton cloth little to achieve." Mr. Duffy indeed, in a postscript to his letter, mentions his having heard a report that the bleachers of *fine* linens had discontinued the use of the lime or potash liquors, fearing their influence on the texture. I can positively state, however, from actual experience, that such a report is quite unfounded. In the neighbourhood of Belfast (the fine linen county) there are few bleach greens in which those liquors are not at present employed to a greater or less extent, as may be consistent with the comparative expenses of the old and new methods. Having been myself familiarized for the greater part of ten years with the bleaching operations as carried on in the north of Ireland, in some of the principal greens, having first attended to chemistry with a view to this art, and been called as evidence upon more than one occasion before the Linen Board, during their investigations respecting the new mode of bleaching,—I can safely state it as the legitimate conclusion from these inquiries and my own experience, that there are no grounds whatever for rejecting the oxymuriatic salts in bleaching, except those arising from the gross ignorance of most of the foremen bleachers, who being altogether unacquainted with the most simple principles of chemistry, cannot possibly have skill sufficient to apply any new agent possessed of much efficacy to their art, without almost the certainty of doing some damage to the goods. The accidents which happened at the first introduction of oxymuriatic acid arose principally from this cause, in conjunction with the blunders of ignorant and designing foreigners who pretended to teach the bleachers.

It strengthens my own opinion materially upon this subject, to find that I coincide in opinion exactly with a gentleman of much knowledge and experience upon the subject, and who has been for a series of years employed by the Trustees of the Linen Manufacture to inspect and direct all the chemical operations of the bleachers. Mr. Higgins also informs me, that not less than sixteen years ago he tried the bleaching powers of the *oxymuriate of magnesia*, found them inferior to the salt of lime, and at once saw the absurdity of proposing a substance so much beyond the reach of the bleacher.

Nothing can be more harmless than to say, that *if* Sir Humphry gives us magnesia for a penny per pound, we will make a trial of his proposal. We may also say, that *if* he gives us *silver* or *potassium* or *sodium* at the same price, we will apply them to many valuable purposes in the arts; that,

that, in short, with this powerful "*if*" we shall accomplish every *desideratum*, and solve every problem in chemistry.

Professor Davy having stated that bits of linen cloth steeped even for a few hours in strong solutions of muriate of lime were *rotted*, and having upon this supposed *fact* grounded his proposal of rejecting the oxymuriate of lime from bleaching,—I shall conclude this letter by stating the result of two experiments out of a great number which I made in consequence, all of which gave similar results.

Experiment 1.—Dec. 16, 1811. Steeped a cutting of full bleached and perfectly *sound* linen, previously macerated in hot water to remove any dressing, in a solution of *neutral* muriate of lime made in the proportion of two ounces of the salt to eight ounces of water.

Note.—The yellowish brown insoluble matter which rose to the surface was removed by filtration, before the immersion of the linen.

17th. Linen taken out of the solution, washed, compared with an unsteeped piece of the same cloth, found perfectly sound, and not perceptibly reduced in its texture. Repeated the steeping in a fresh solution somewhat increased in strength.

Note—After twelve hours immersion of the lime, tested the solution with litmus, and did not discover any disengaged acid.

18th.—Linen examined as before, no change, washed, dried, and repeated the steep in a fresh solution of the same strength.

19th—Linen perfectly sound, repeated the steeping.

20th.—No perceptible change; the linen resisted equal efforts to tear it, as the unsteeped piece.

Experiment 2.—Dec. 28.—Steeped another cutting of full bleached *sound* linen in a solution composed of two ounces of muriate of lime to four ounces of water.

29th.—Linen taken out of the solution, washed, examined, and found unaltered. Repeated the steeping in a fresh solution similar to the last.

30th.—No change in the texture of the linen, steeped as before.

31st.—Linen not perceptibly reduced, or weakened, in comparing it with some which had not been in the solution.

The solutions used in these experiments were at least 500 times the strength that they ever occur in the common process of steeping in the oxymuriate of lime, and the linen used had undergone the full process of bleaching: by which it was to a certain degree already reduced, and its colouring matter so completely separated as not to impede the action of any *corrosive* agent upon its fibres. Is it not then more than probable, if in any instance sound cloth is tendered in the usual process with the salt of lime, that it must either be owing to an error in using too strong a solution of the salt, to the presence of free muriatic acid, or to negligence in the after treatment of washing, souring, &c.?

Need I ask, after such experiments and results, what degree of credit should be given to the unqualified assertion, that even a *single steeping* of *sound* linen in muriate of lime was sufficient to rot it!!

Could I see the smallest relation there is between this question and the bleaching art, I would certainly think it my duty to bring the subject before the Linen Board, and request the honour of performing the experiments in their presence, being satisfied what would be the result. My former results were submitted to and examined by several members of the Kirwanian Society. Since that time the subject has been taken up by several persons in this place. A very intelligent gentleman, Mr. S. Witter, of Chester, assures me, that he has steeped both linen and cotton for five days in very strong solutions of muriate of lime, and found their texture perfectly unimpaired. This gentleman informs me at the same time, that upon making several trials of different specimens of the *oxymuriate of lime* of commerce, he finds only 87 grs. of the real salt in 240 grs. From this it is easy to calculate, that in the bleacher's steeping liquor there never can exist one part of muriate of lime to 2000 of water; such a state of dilution, that, if possessed of all the energy of caustic potash in its state of concentration, it could not affect the most delicate substance. Mr. Donovan* has also at my request repeated the experiment with every possible precaution, and found a result similar to mine. Here then is assertion opposed to assertion, fact to fact: who then is to decide? to which side does the suspicion of inaccuracy and unfairness incline? This must be left to those who are judges of such subjects, and who will think for themselves.

I am, sir, with much respect,

Your obedient servant,

Dublin, August 22, 1812. JAMES OGILBY.

* This is the gentleman who assailed Professor Davy's favourite hypothesis of the identity of chemical and electrical attraction. — Vide Phil. Mag. vol. xxxvii.

XXXVI. *An*

XXXVI. *An Attempt to analyse Silica. By Professor* Berzelius*.

The attempts hitherto made to decompose silica by means of the electric pile having left the problem undetermined, whether it can be reduced to a metalline body like other earths, I attempted to gain an analogous result by mixing it with powdered charcoal and filings of iron, and exposing the mixture, in a luted crucible, to a degree of heat sufficient for the fusion of the iron; hoping that the iron might in this case act somewhat similarly to quicksilver with the electric pile.

a. Three grammes of iron filings mixed with $1\frac{1}{2}$ gramme silica, and 0·66 of a gramme powdered charcoal, were put into a small crucible covered and luted in the ordinary way, and exposed to the heat of a blast furnace for one hour. The mass when cold was taken out, the small reguli of iron were extracted by the magnet, and rubbed with the palm of the hand against paper, until, by continued rubbing on new and clean paper, they ceased to soil the paper. They had a silver colour; some of them admitted of being flattened; others were brittle, according as they had taken up more or less carbon. 1·5 gr. of these polished metallic globules were then put into sulphuric acid diluted with six times as much water. They remained undissolved till heat was applied, when they dissolved slowly. When all action had ceased, the forms of the globules still remained, but changed in colour. Some were white as snow; others black, and much like plumbago. When these were afterwards burned in an open fire, there remained siliceous earth of the form of the metallic globules. Those that had been white were still white, and those that had been black were now pale red. This earth amounted to $3\frac{1}{2}$ per cent. of the weight of the ferruginous globules.

b. Having perceived the probability that these white skeleton globules of silica were derived from the soft reguli, and the black from those most charged with carbon; I determined to try whether a silicated iron, tolerably free from carbon, might not be obtained by using less charcoal. I therefore mixed equal parts of fine powder of flint and iron-filings with $\frac{1}{20}$th of their weight of pulverized charcoal, using a little mucilage of gum dragon to form a paste, of which little balls were made. These packed up in fine powder of flints were exposed to the blast in the same manner

* From *Afhandlingar i Fysik, Kemi och Mineralogi, utgifne af W. Hisinger och F. Berzelius.* 3 H. Stockholm. 1810.

ner as the first experiment. When cold the silica was found unaltered, pale, and almost white. The reguli were not so fully fused as in the former experiment, but had caked together here and there, in the form of lumps. The most globular reguli were selected and polished, and of these 1·15 grammes were dissolved in aqua regia. The dissolution proceeded with great violence, and left silica of the form of the balls, of which some few were brown. Aqua regia and the nitric acids are such sensible reagents for carbon in silicated iron, that from the same iron where muriatic and sulphuric acids leave a white earth, they produce it brown, and sometimes, when the nitric acid is used, black. The silica after ignition was snow white, and retained the form of the iron. It weighed 0·225 of a gramme, or about 19 per cent.

c. Part of the reguli obtained in the foregoing experiments were stretched out to thin laminæ, and then dissolved in aqua regia. The dissolution was vehement, and the acid left a white mass of silica in the form of the laminæ, which swelled by a continued digestion, and, when the acid was concentrated by evaporation, became semi-gelatinised. This experiment proves that the malleable iron mass was more free from carbon than the unmalleable, and that it nevertheless contained the base of the silica, which therefore had had no influence on its softness, which seemed entirely to depend on the more or less complete deprivation of the carbon.

From these data, I formed the conclusion that silica is by means of carbon reduced to a body, which enters into union with iron, and which, not injuring the malleability of the iron, must be of a metalline nature.

In opposition to my conclusion, some affirm that the silica is only mechanically mixed with the melted iron. As this opinion directly contradicts every notion that I can form of the usual habitudes of melted metals with those pulverulent bodies on which they exercise no chemical affinity, I tried to cement the filings of iron with silica and powder of charcoal, at a temperature which could not fuse the iron. The iron filings remained unaltered in point of form, but, on dissolution in muriatic acid, left as much as 6 per cent. of siliceous earth.

d. In order to determine in some degree what quantity of combustible siliceous base had been appropriated by the iron in these experiments, I dissolved 3·5 grammes of silicated iron in concentrated muriatic acid, and secured the gas in an apparatus suitable for its combustion. The gas was

was collected over water previously made use of in several similar experiments with malleable iron and cast iron, in hope that it would take up less of the evolved gas, being previously saturated. Burnt in pure oxygen gas, free of carbonic acid, over lime water, it produced 0·7 of a gramme of carbonate of lime which contained 0·305 gr. of carbonic acid, which according to Allen and Pepys's experiments correspond to 0·087 of carbon. The solution with muriatic acid left a gray siliceous powder, which was collected on a weighed filter, and when dried (in a vessel secured from access of air) in a dark red heat weighed 0·355 of a gramme. Heated to white in an open crucible, it lost 0·02 of a gramme more, and became white. When these are added to what was before obtained, the whole quantity of carbon proves to be 0·107 of a gramme, or 3 per cent. of the weight of the iron. I oxygenated the solution of iron by boiling it with nitric acid, and precipitated with caustic ammonia. The strongly ignited precipitate weighed 4·71 grammes, corresponding to 3·266 grammes of iron. When the iron and the carbon (3·266 + 0·107) are added together, we have 3·373. The siliceous base will therefore be 0·127 gr. These 0·127 had produced 0·335 of a gramme of silica, and of course had taken up 0·208 oxygen. The silica therefore appears to be composed of 38 parts base and 62 parts oxygen. This quantity of oxygen in the silica, although agreeing well with its insolubility in acids, on applying the rule that the metallic oxides, in proportion to their charge of oxygen, are less soluble in acids, was unexpectedly great, and led me to suspect that, in the treatment of the gas, something might have escaped my notice, and rendered the charge of carbon too high.

e. Ten grammes of silicated iron of a mixed nature, partly malleable and partly like cast iron, produced, during a dissolution in concentrated muriatic acid, 163 cubic inches of gas, including the air contained in the vessel. The last portion of gas was expelled by boiling. The water had a disagreeable smell, from adherent new formed oil, showing a loss of carbon which could not be calculated. The gas burned in oxygen gas over lime water yielded 0·782 carbonate of lime, corresponding to 0·341 carbonic acid, and 0·097 carbon. The solution left a white, light, and porous siliceous earth, which after a strong drying weighed 0·665 gr. By ignition it at first blackened, but on continuing, the heat became white, and lost 0·02. I consider two-thirds of the loss to be carbon, since the earth before the ignition was without smell and colour, and since the oleaginous

oleaginous mass had been for the most part dissipated by boiling. The quantity of carbon is therefore 0·0985. The solution of iron oxygenated with nitric acid yielded 13·745 grammes of red oxide of iron*, answering to 9·53 grammes of iron. The water employed in washing had a faint blueish colour, owing to some copper which, notwithstanding a careful separation with the magnet, had adhered to the iron filings. When evaporated to dryness and ignited, the saline mass left a gray brown substance weighing 0·03 of a gramme, from which muriatic acid extracted 0·01 oxide of copper, corresponding to 0·008 of copper. The remainder was silica 0·02 gr., which with that before obtained gives 0·665 of silica. If now we add the iron to the carbon and the copper, the sum will be 9·53 + 0·0985 + 0·008 = 9·6365. The deficiency 0·3635 of a gramme must be base of silica, which had afforded 0·665 of a gramme of silica. Silica according to this experiment consists of 54·66 base, and 45·34 oxygen. But, as we have here a manifest loss of carbon, the result cannot be decisive; and if we assume that the iron on an average contained 0·012 of its weight of carbon, (reckoned according to the proportions between the quantity of carbon obtained when the gas from dissolved cast-iron is burnt, and that which the iron by experiments is found to contain, and which hereafter will be described,) then the quantity of base will be 51·5, and of oxygen 49·5.

f. I tried to bruise a part of this silicated iron in a cast-iron mortar, in order that I might afterwards burn it in fusing nitre, as in the analysis of cast-iron, to find out the whole charge of carbon; but it was too ductile to be pulverized. Nevertheless, mixed with nitre, I heated it in an apparatus like that described for the analysis of the cast-iron, but no carbonic acid was disengaged; and when, after an ignition of three hours, only a small quantity of oxygen gas was obtained, I discontinued the experiment. I dissolved the mass in water, which produced with lime water no sign of precipitation. The iron had blackened on the surface, but was in other respects unaltered. Since in this manner it was impossible to acquire certainty respecting the quantity of carbon, I now endeavoured to obtain a carburet of cast-iron, by means of iron filings free from copper, mixed with double its weight of silica and powder of charcoal. This was expected to contain no malleable iron; but I found that this also would not admit of being

* Dissolved in muriatic acid, it left scarcely perceptible traces of silica in a half gelatinated state.

pulverized

pulverized sufficiently fine, owing probably to the slow cooling of the crucible. I therefore mixed it with clean powder of flints, and attempted by means of a new fusion to get it malleable with an increased charge of silica, when the carbon of the cast iron should have reduced the earth. The silicated iron obtained was soft, but intermixed with more or less malleable iron, as in the preceding experiments.

g. 4·5 grammes of silicated iron from that so prepared, being well cleansed, were dissolved in diluted muriatic acid by means of such apparatus as was used in the preceding experiments. The solution was boiled towards the conclusion, for the purpose of expelling all gas out the vessel, which, together with the atmospheric air out of the apparatus, made 77 decimal cubic inches. This amounted to near three cubic inches more than what an equal weight of bar-iron had produced, when dissolved in muriatic acid in the same apparatus. This indicates that the silicated iron did contain a body which absorbed more oxygen than pure iron, and so much the more, as the great charge of carbon would have presupposed a less quantity of disengaged gas. The gas on burning over lime water left 0·3075 gr. carbonate of lime, answering to 0·134 of carbonic acid, or 0·0381 of carbon (0·846 per cent. of the weight of the iron, nearly as much as in the preceding experiment). The acid left undissolved a dark siliceous earth, which after drying was of a light gray colour, and weighed 0·578 gr. During ignition in a closed vessel, it disengaged a gas of an empyreumatic smell, which in combustion burnt with a blue flame. By this ignition the earth lost 0·013 of a gramme, but did not become white. Carbon was found in that which was expelled, and I think I cannot be much mistaken if I set it down at 0·005 gr. The grayish siliceous earth, when ignited for half an hour in a half covered platina crucible, suffered no diminution of weight, but did not acquire a full whiteness. The solution of iron, oxidated with nitric acid, was precipitated with caustic ammonia, and yielded 6·0 grammes of red oxide. Dissolved in muriatic acid, it left 0·015 of a gramme of silica; and from the water employed in washing was obtained, after evaporation and the ignition of the salt, 0·0075 of a gramme more of the same earth; so that the whole quantity of silex became 0·5875 of a gramme. If from 6 grammes of red oxide of iron 0·015 of a gramme of siliceous earth be deducted, there will remain 5·985 grammes, corresponding to 4·15 grammes of pure iron, which being added to 0·0431 of carbon, make 4·1931 grammes. These deducted from 4·5 grammes, there remain 0·3069 for base of of silica. It had produced 0·5875 of a gramme of silica, and had therefore taken up 0·2806 of a gramme of oxygen. This experiment differs by about 2 per cent. from the foregoing; but both have this fault, that a portion of carbon is lost, as a component part of the fœtid oil wherewith the water was impregnated.

5. Having seen the impossibility of obtaining a rigid result by means of solutions of iron, I undertook to incorporate the base of silica with copper, by means of a similar process of reduction with powder of charcoal. The silicated copper was dissolved in nitric acid, during which there existed evident marks of the presence of carbon, which at first separated in the form of a brown powder; but vanished, whilst the departing gas, conducted through lime water, deposited a portion of carbonate of lime. The copper dissolved to a clear fluid, which on cooling thickened to a blue jelly, and left after drying and washing a gray siliceous earth, amounting to 5 per cent. of the weight of the copper. Attempting to separate the copper with so much nicety as to produce a more exact result than the preceding, circumstances occurred which rendered a loss inevitable, and thereby made this experiment still more uncertain.

I have not tried to unite the base of silica with silver by fusion with carbon, but I have no doubt of its success. It will probably present a more accurate result, although the silver will also yield a sensible quantity of carbon from its solutions in nitric acid*. As to the determination of the precise quantities of oxygen and base forming silica, I consider this as only of secondary importance: it is at present sufficient, that these experiments determine with a tolerable certainty that silica, mixed with powder of charcoal, can be reduced in fusion with a metal, and that its base, when the metal is dissolved in acids, is, by the absorption of about an equal weight of oxygen, again converted into silica.

XXXVII. *On the Luminous Appearance of Sea Water.* By KNIGHT SPENCER, *Esq.*

To Mr. Tilloch.

SIR, THE luminous appearance which the sea (when agitated)

* This experiment with silver has since been performed by the German chemists; and the result, as I have been informed, was such as Professor Berzelius has here anticipated.—EDIT.

agitated) occasionally exhibits, and the desire satisfactorily to account for this phænomenon, have caused much investigation; but throughout the whole I do not recollect that any thing like the phænomena stated below has been observed.

Believing then the fact to be new, I venture to request you will give it a place in your Magazine; and I indulge the hope that it may lead to a further and profitable discussion of the subject.

On the 8th of August last, in the evening, being at Blackpool on the western coast, and observing the sea to be at times very luminous,—I went to the beach, in order to view it more clearly. And accidentally stamping on a flat part where the tide had flowed, but from which it had receded a few feet, it being about half an hour after high water, I was surprised with the appearance of some hundreds of sparks of light, which surrounded my foot in every direction, to the distance of about 15 inches, the largest being nearest. The light, however, was only instantaneous.

Struck with the novelty of the appearance, I stamped a second time on the same spot, and produced a very few sparks; I afterwards stamped repeatedly on the same spot, but without producing any. I then sought for a similar flat spot on the beach; and, stamping upon it, I produced the same appearances as in the first case. I then tried to elicit sparks by stamping on the sloping part of the beach which inclines to the sea, but without success: on trying again, however, on the flat parts, the same appearances invariably occurred.

Unwilling that so singular a fact should rest on the dictum of an individual, I requested L. Starkie, esq. of Huntroyd, in that neighbourhood, to accompany me to the beach; where we repeated the experiment several times with the same success.

It seems therefore that the tide at high water had deposited something in these flat places; and that the friction of the shingles against each other, produced by the stamping, was the cause of this matter emitting the luminous appearances: the tide had effectually left the beach, and there was not the least appearance of puddles of water in the places acted upon.

I am, sir,

Your obedient servant,

KNIGHT SPENCER.

Surry Institution,
Sept. 2, 1812.

XXXVIII. *De-*

XXXVIII. *Defence of Professor* Leslie *against the Edinburgh Review.*

To Mr. Tilloch.

Sir, It appears from the last Edinburgh Review, No. 39, pp. 89, 90, 91,) that Mr. Leslie has impugned a certain mode of reasoning used by Legendre, which on the other hand the reviewer strenuously defends; and those who admire the most what they understand the least (a numerous class certainly) may perhaps think him in the right.

He who denies that a line can be a function of an angle (because, according to him, an angle is of the nature of number, and of no dimension,) must surely be ignorant of a curve called the *spiral* of Archimedes, of which it is the very nature that the polar ordinate is directly as an angle. Perhaps, however, this curve must be classed with impossible quantities.

I confess, sir, the *postulate* of the reviewer appears to me to be mere *verbiage*, and gratuitous silliness.

"The quantities *A*, *B*, *C*, are angles; they are of the same nature with numbers, or mere expressions of ratio, and, according to the language of algebra, are of no dimension."

The strange and unsupported assertion that angles are of the same nature with numbers, can only be accounted for from his confounding* (in an inquiry in which, more than in any other, such things must *not* be confounded) an angle, with the cosine divided by the radius.

That an angle is of *no dimension*, if it simply mean that an angle is not a line, nor a plane, will hardly be disputed; but then, on the other hand, a line is not an angle, nor angular space of any kind; so that the argument applies as much on one side as the other.

If the assertion mean *more* than this, it is not easy to perceive its truth; for, as a line is considered of one dimension, when compared with a plane; so may an angle be considered of one dimension, when compared with "the angular space subtended by a surface," this having two dimensions. If, therefore, there are different dimensions of space, angular space has also *its* different dimensions; and the heterogeneity is reciprocal. Nor can an angle be of the nature of number any more than a line; there must be an arbitrary choice of an unit in both cases.

* In the equation $\frac{\cos. C}{rad.} = \frac{a^2 + b^2 - c^2}{2ab}$; which he calls an equation between C and a, b, c.

Leaving

Leaving this Platonist*, let us turn to Mr. Leslie's reasoning, and consider how far he is right, and where he deviates from the true line of argument.

This author justly observes, that the same reasoning which proves that there can be no *equation* between c and $A, B, C,$ would equally prove that there can be no *equation* between C and $a, b, c.$ Mr Leslie might have gone further; for we may without hesitation admit, that the reasoning is *just* in *both* cases (provided that we keep to the strict meaning of the word *equation*), and yet *proves nothing in either*.

For the argument drawn from the heterogeneity of the quantities no longer applies, when transferred from *the angle itself* to such functions as the sine, cosine, or arc to a given radius †; these are lines, and of course capable of a comparison with lines; whilst they can *not* be compared with the angles themselves of which they are called the functions. Hence appears the utter uselessness of Legendre's reasoning. It is true, sir, you have shown that there can be no *equation* between c and $A, B, C;$ but if $\alpha, \beta, \gamma,$ represent the cosines of $A, B, C,$ to any given line (r) as radius, I do not very clearly perceive (*from any thing you have delivered*) why there might not be such an equation between the five lines $c, \alpha, \beta, \gamma, r;$ as would entirely overturn all you mean to establish. There might, for example, be the equation $c = \frac{\alpha^2.\beta^2}{\gamma r^2}$; whence $\gamma = \frac{\alpha^2 \beta^2}{c r^2}$; and the angle C would *not* depend on A and B alone, as you think you have demonstrated.

Sept. 14, 1812. X. Y.

P.S.—I must state that I have never seen Mr. Leslie's Geometry, and know nothing more of his argument than is to be gathered from the Edinburgh Review.

XXXIX. *On some Combinations of Platina.* *By* Edmund Davy, *Esq., of the Royal Institution. Communicated by the Author.*

INTRODUCTION.

The properties which characterize platina, and to which it owes its value, offer many difficulties to the complete

* Vide Edinburgh Review, October 1809, p. 4.

† Let it be particularly observed, that I am here speaking geometrically, and do not mean the sine, &c to the radius one: this is a number, and supposes a previous choice of some line for unit.

development of its combinations. Hence its chemical history is perhaps less known and more limited than that of any other metal. In the Philosophical Magazine for July, I have described some combinations of platina with sulphur and phosphorus; in the present communication, I shall enter into some details relative to some other compounds of this metal. I shall also venture, on the authority of experiments, to controvert some statements respecting the oxides and salts of platina. I have to regret that other engagements will not allow me leisure to complete the present, together with other investigations I had begun; but I shall rather choose to bring them occasionally forward in an imperfect state, than to withhold them entirely, in the anticipation of a period which perhaps may never arrive.

1. *Of the Hydrosulphuret of Platina.*

The compounds obtained by the agency of sulphuretted hydrogen gas on acid solutions of the metals are but imperfectly known, they have not yet been examined with precision; hence, as might be expected, there exists among Chemists a diversity of opinion concerning their constitution. A minute investigation of this class of bodies would serve to elucidate many chemical phænomena relating to the metals, and, there is every reason to believe, would furnish novel and interesting results. It is not my intention to enter into any details relative to the nature of these bodies in general, but simply to confine myself to the consideration of the hydrosulphuret of platina.

This substance may be obtained, by treating a solution of platina with water impregnated with sulphuretted hydrogen gas; or by passing a current of gas through the solution. After the gas has been passed for a short time through the solution, the surface is covered with a thin coating of a dark iron-gray colour, having the metallic lustre; a quantity of it is also deposited at the bottom of the vessel. As it readily undergoes chemical change in the atmosphere; when it is wanted for accurate experiments, the best mode of procuring it seems to be that I adopted, which will presently be mentioned.

The hydrosulphuret of platina has been partially known for some time; but it has not I believe been described, nor its composition determined. I do not think it has been at all examined in a state of purity. It has been considered by some chemists as metallic platina; and Dr. Thomson in his System of Chemistry states it as a distinguishing character of platina, that it is precipitated by sulphuretted hydrogen

drogen in the metallic state. M. Proust considered it as a sulphuret, and first noticed a singular property it has of forming sulphuric acid whilst drying in the atmosphere*. This fact has since been observed by Professor Berzelius of Stockholm, as he lately informed me. In operating on this substance, I soon had occasion to verify the preceding observation of those able chemists; for in cases when it was dried on paper, at the moderate heat of a sand-bath, the paper was burnt by the acid and entirely destroyed. When it was dried at the common temperature of the atmosphere, a quantity of acid was also formed. It may not be improper briefly to describe this substance after it has been dried at a gentle heat in the air, and when of course it has undergone partial decomposition.

Its colour is black; it is in small pieces, the particles of which are loosely coherent. It is destitute of lustre. It marks the fingers or paper, but the lustre is much inferior to that of plumbago. It has a strong acid taste. It is destitute of smell. When heated just below redness on a thin slip of platina, it deflagrates, emitting red sparks, and is partially decomposed; by exposing it to a red heat for some minutes, sulphureous fumes are copiously emitted, and the platina remains in a state of purity. It appears to be unaffected by boiling muriatic or sulphuric acid. It is soluble with some little difficulty in boiling nitro-muriatic acid. It is rapidly decomposed, with vivid ignition, when heated with oxymuriate of potash. When it is heated in close vessels over mercury, the products are water, sulphureous acid gas, a minute quantity of sulphur, and a substance analogous in its physical and chemical properties to the subsulphuret of platina which I have already described. I shall state one experiment which seems to prove this fact. Some hydro-sulphuret which had been drying for some days on a sand-bath in a platina crucible, was heated to redness in a retort over mercury: during the process it became partially ignited: the products were similar to those above mentioned: ten grains of the residual substance were decomposed at a red heat in a platina crucible, and furnished 8·5 grains of pure platina. This gives its compsition 85 platina,

15 sulphur,

———

100;

and if allowance be made for a minute quantity of sulphur

* *Annales de Chimie*, tome xlix. p. 180.

expelled at the moment of ignition, it almost precisely agrees with the subsulphuret.

As the hydrosulphuret absorbed oxygen, became acid, and attracted water when dried in the atmosphere; it is evident that in such a state it could only furnish variable results, depending entirely on the circumstances to which it had been previously exposed. To obviate these difficulties, it was necessary to have recourse to some mode of obtaining it, by which the agency of the atmosphere would be excluded. As I knew no practicable method of separating the acid from the hydrosulphuret and edulcorating it with distilled water, out of the contact of the air, it became necessary to determine with some degree of precision the agency of air and water on it. With these views, I exposed a surface of recently made hydrosulphuret of platina (which had been well washed with distilled water, and hastily dried with bibulous paper,) on a platina cover, to a known quantity of oxygen gas; after 20 hours the gas was scarcely diminished, and after about 10 days only $1\frac{2}{10}$ths of a cubical inch were absorbed.

Some of the above hydrosulphuret was also put into a bottle filled with distilled water; the water was occasionally examined, but after some days no sensible quantity of sulphuric acid had been formed.

These experiments seemed to prove that this substance might be washed in contact with the atmosphere, without undergoing any perceptible change in its constitution. I shall now state the mode I adopted to procure it. Into a retort of about the capacity of 40 cubical inches, an aqueous solution of muriate of platina was introduced; it was exhausted of air, and filled with sulphuretted hydrogen gas; a considerable absorption instantly took place, which was much increased by agitation; fresh portions of gas were occasionally admitted, and the process was continued until the fluid became nearly colourless. By this means all the platina may after some time be separated from the solution. The hydrosulphuret precipitated was of a uniform black or very dark brown colour, and exhibited in some places, especially the film on the surface, the metallic lustre. It was now put on a filter, and washed with distilled water till this fluid was tasteless, and did not affect litmus paper; care being taken to keep it during this time always covered with water. Whilst it was yet quite moist and the water still oozing through in drops, it was hastily put into a tubulated retort, to which a stop-cock had been previously

previously attached; the retort was exhausted twice, and filled with hydrogen or carbonic acid gas. It was exposed to the heat of a sand-bath, and the stop-cock opened under water. After some time the vapour of the water expelled the gas in the retort; when this was effected, the stop-cock was closed, and the neck of the retort kept cold by wet cloths. When the hydrosulphuret had been thus heated in a partial vacuum for about 10 hours, it appeared to be quite dry, and no indications of moisture could be perceived in the bulb of the retort. When cold it was slowly opened over hydrogen gas. In one experiment in which the retort had been previously filled with hydrogen gas, nearly a half of a cubical inch of water was obtained, which had the smell of sulphuretted hydrogen gas, and produced a dark-coloured precipitate with nitrate of lead. I intended to have examined this water more minutely, but was prevented by an accident. It seems probable that it was merely impregnated with a little sulphuretted hydrogen gas, expelled by the agency of heat. In another instance when the hydrosulphuret was procured, the retort being previously filled with carbonic acid gas, the water that came over did not contain any sulphuretted hydrogen gas. I cannot with certainty state the greatest temperature to which the hydrosulphuret had been exposed in these experiments; it was necessarily variable: but from some trials I made with the thermometer, I have reason to think it could scarcely at any time have exceeded the boiling point of water. A well stoppered bottle was filled with the dry hydrosulphuret, and it was immediately examined.

Physical and Chemical Properties.

Its colour was dark brown. It was in small flat lumps, which were readily reduced to powder. Its external surface exhibited a feeble lustre, which was no longer apparent when pulverized. Its lustre was scarcely inferior to plumbago when it was rubbed on the fingers or paper. It had no smell. It was insipid when first brought in contact with the tongue; but as it became divided in the mouth, a tolerably strong saline taste was perceived. It scarcely affected litmus paper. When it was put into water, a slight crackling noise was produced, and small bubbles of gas were disengaged. 3·2 grains of hydrosulphuret afforded in this way $\frac{15}{100}$ of a cubical inch of gas, which diminished considerably with nitrous gas, and did not burn or explode with a taper. I expected to have found this gas hydrogen,

 and

and the effect to be connected with the production of sulphuric acid. I was surprised to find that it exhibited the properties of oxygen. I cannot account for this phænomenon; it can scarcely be owing to the absorption of common air, the volume of gas being about eight times greater than that of the hydrosulphuret. When small lumps of it are introduced into nitric acid, a slight detonating noise is produced, and the substance instantly falls to pieces. These effects are connected with the decomposition of the nitric acid and the formation of sulphuric acid, and are much increased, with the rapid disengagement of nitrous gas, on the application of a gentle heat. When a little of the hydrosulphuret is put into concentrated sulphuric acid, gas is disengaged from it: on heating it, a hissing noise is produced, and gas copiously evolved; but it seems to be quite insoluble in this acid. Muriatic, phosphoric, and acetic acids occasion a disengagement of gas from the hydrosulphuret; but the action of these acids on it is very limited, and attended by no other effect worthy of notice. I have not examined the gas evolved by the agency of these acids on the hydrosulphuret. The quantities on which I have operated have been too small for this purpose. When it is introduced into a strong solution of pure potash or ammonia, a hissing noise is produced; but there is apparently no further effect. When it is exposed to the atmosphere, it absorbs oxygen, and sulphuric acid is formed. Five grains of it exposed in small lumps for four days gained more than $\frac{7}{10}$ths of a grain in weight, and this increase was in a gradually diminishing ratio, in proportion to the time. It then furnished, by its decomposition in close vessels, sulphureous acid gas, water, and sulphuret of platina. When the hydrosulphuret is heated in nitrous gas, it becomes an ignited mass; when oxygen gas and moisture are present, a white crystallized substance appears to be formed, which furnishes nitrous gas and sulphuric acid by the agency of water. I have not minutely examined these results. Nitrous acid vapour acts spontaneously on the hydrosulphuret, occasions a hissing noise, and the formation of sulphuric acid. When the hydrosulphuret is heated to redness in close vessels over mercury, the products are sulphuretted hydrogen gas, with a little sulphureous acid gas, water, sulphur, and sulphuret of platina. When it is introduced into chlorine gas, an immediate action is produced, and copious white fumes are evolved; these effects are much increased by the agency of heat, and a substance of a brown and blackish colour is obtained which gives off gas, and is for the most part soluble

ble in water. It appears to be a muriat of platina with a little undecompounded hydrosulphuret.

All these experiments were performed on minute portions of the hydrosulphuret: it would be very desirable to repeat them on larger quantities.

From the foregoing statements, it is evident that the substance obtained by the agency of sulphuretted hydrogen gas on a solution of platina is not a true sulphuret of this metal. On a comparison of their chemical properties, it will appear that they are decidedly different. Nor can it be, it would seem, a compound of sulphuretted hydrogen gas with oxide of platina, for in this case no sulphuretted hydrogen gas would be furnished by its decomposition in close vessels; water and sulphureous acid gas would be the only results. Now in the actual products water and sulphureous acid gas are found, but in such small quantities, especially the latter, that oxygen and water can only be considered as accidental, and not at all essential to the constitution of the hydrosulphuret. They may fairly be presumed to originate from other sources. The hydrosulphuret was dried at a temperature scarcely exceeding that of boiling water, and there can be no difficulty in admitting that it still contained water. As it absorbed oxygen by exposure to air, and as in the course of the experiments it must necessarily have been exposed for a short time to this agency, the production of a small quantity of sulphureous acid gas is easily accounted for.

The actual results seem to prove that this substance is composed of sulphuret of platina and sulphuretted hydrogen, or of platina, sulphur, and hydrogen; and the facts seem to admit of an easy solution on this idea. The sulphuretted hydrogen may be presumed to be in a loose state of combination with the sulphuret. Hence, by exposure to the atmosphere, oxygen is absorbed, sulphuric acid and water are formed, and a sulphuret of platina remains which is not affected by the air. The sulphuric acid formed by exposing the hydrosulphuret to the heat of a sand-bath for several days is quite colourless with water; the effect seems to be wholly unconnected with the solution of any of the platina. I have not yet ascertained the precise limits of the decomposition of the hydrosulphuret by the agency of the air; but it seems highly probable that it will be found to be only in proportion to the quantity of sulphuretted hydrogen it contains. The sulphurets of platina undergo no apparent changes by being exposed to the atmosphere, at least, for several days.

Analysis.

The method I employed to ascertain the component parts of this substance was of the simplest kind, but I know of no mode which offers more correct results. It is founded on a property, which, in the class of metallic hydrosulphurets, I believe, almost exclusively belongs to those of platina and gold; namely, that of being decompounded by the agency of heat and air, the metal alone remaining in a state of purity. I made many experiments on the hydrosulphuret of platina before I obtained any satisfactory results. Those which nearly agree, and which seem to afford evidences of its constitution, I shall immediately relate.

Experiment 1. Ten grains of hydrosulphuret of platina were heated in a small retort over mercury, gas was soon disengaged, and a little limpid fluid condensed in the neck of the retort; the heat was continued for several minutes, and when the retort had acquired a dull red, the substance became partially ignited *, a copious evolution of gas immediately ensued, and a little yellow sublimate was deposited. When the gas ceased to come over, and no further change could be produced by the utmost heat of a spirit-lamp, the retort was suffered to cool, and the results were carefully examined. The gas obtained, allowance being made for the common air, and corrections for temperature and pressure, amounted to 1·25 of a cubical inch, and was almost entirely sulphuretted hydrogen gas, with a minute quantity of sulphureous acid gas. On agitation it was absorbed by water, and instantly produced a dark-coloured precipitate with a solution of the nitrates of lead and silver. It had the precise odour of sulphuretted hydrogen gas, and inflamed with a lighted taper in contact with the atmosphere, depositing sulphur on the sides of the tube. The limpid fluid was water impregnated with sulphuretted hydrogen, and produced similar effects on metallic solutions as those above stated. The yellow sublimate was a slight film of sulphur, and weighed about $\frac{4}{10}$ths of a grain. This estimate of the sulphur must be considered merely as an approximation, as it was in a moist state. The precise quantity of water and sulphur thus obtained could not be determined with precision; but they could scarcely together have exceeded a grain in weight. The fixed substance at the bottom of the retort weighed 8·3 grains, and in its phy-

* This phænomenon of the production of fire, accompanied by the separation of one of the constituent parts of the hydrosulphuret, appears to be perfectly analogous to that observed by Sir H. Davy in the decomposition of euchlorine gas at an elevated temperature.

sical

sical characters strictly resembled the supersulphuret of platina. From the experiments I have made on it, which will hereafter be noticed, it appears to be a true sulphuret of platina containing 22 per cent. of sulphur.

Experiment 2.—Ten grains of the same hydrosulphuret, heated as in the above experiment, furnished similar results; the quantity of gas was precisely the same. The sulphuret obtained weighed 8·25 grains.

Experiment 3.—Twelve grains of hydrosulphuret, which in drying had been exposed to a higher temperature than that used in the preceding experiments, afforded similar products; but the sulphuretted hydrogen gas was less in quantity, it did not amount to a half of a cubical inch. This deficiency must be referred to the increased temperature, by which the gas was expelled and found in the water, as has been before noticed. The proportion of sulphureous acid gas was small, but the odour of it was more perceptible; there was also less water and more sulphur than in the experiments detailed. The quantity of sulphuret of platina obtained amounted to 9·85 grains, which, on being decompounded at a red heat in a platina crucible, afforded results varying but little from those previously procured.

From these experiments we shall probably gain near approximations to the true composition of the hydrosulphuret of platina. They all closely agree as to the quantity of sulphur and platina, but differ as to the proportion of gas obtained; which has been accounted for. Of the three experiments above detailed, the first two appear to me to be most worthy of confidence. As the hydrosulphuret had in the first experiment been least exposed to the agency of the atmosphere, and consequently would furnish more correct results, I shall venture to make its data the basis for determining the proportions of its constituent parts. From this experiment the following results are derived:

10 grains		grains.	or, in 100 grs. grains.
	Sulphuretted hydrogen 1·25 cub. inch	= 0·456	— 4·56
	Sulphuret of platina	8·300	—83·00
	Sulphur	0·400	— 4·00
	Water impregnated with sulphuretted hydrogen	0·844	— 8·44

I have estimated the weight of 100 cubical inches of sulphuretted hydrogen gas at 36·5 grains. It is derived from Sir H. Davy's statements, who says that 100 cubical inches weigh between 36 and 37 grains.

The sulphuret of platina contained 22 per cent. of sulphur; consequently the above 83 grains consisted of 64·74 platina

platina + 18·26 sulphur. I found that a cubical inch of distilled water, the barometer being at 30°, and thermometer at 60°, absorbed three cubical inches of sulphuretted hydrogen; hence the eight grains would absorb about 0·34 of a grain; which added to the 4·56 grains = 4·9 grains. Hence, 100 grains of hydrosulphuret of platina afforded

Platina	64·74
Sulphur.......... 18·26 + 4 =	22·26
Sulphuretted hydrogen gas about	5·00
Water	8·00
	100·00

The most probable idea of its constitution is perhaps that it is compounded of the supersulphuret of platina and sulphuretted hydrogen*, and the results appear to agree very nearly with this supposition. According to my experiments in the Philosophical Magazine for July, the supersulphuret of platina contains 28 per cent. of sulphur. Now 100 grains of the hydrosulphuret appear to contain 64·74 grains of metallic platina, which would require 23·24 grains of sulphur to be converted into the super-sulphuret. The actual quantity obtained, amounts to about 22·26 grains. These coincidences were rather unexpected, and are much greater than could have been anticipated in experiments of this kind.

In the course of my experiments on the hydrosulphuret of platina, I had occasion several times to observe a brown precipitate produced in a solution of platina by sulphuretted hydrogen in particular circumstances, especially when it was made over mercury, by letting up a solution of platina into a jar of the gas. I made some unsuccessful attempts to examine this substance, thinking its constitution might be different from the hydrosulphuret I have described. I found it rapidly changed colour in contact with air, and became black: probably its composition may be determined by comparative experiments on the quantity of gas necessary to produce it. Should the results I have obtained be confirmed by future experiments, we shall probably become acquainted with a new class of compounds of which nothing is at present known, consisting of

* Since my paper went to press, I made the following experiment, which, if it does not confirm the above idea, gives it a still higher degree of probability. A cubical inch of an aqueous solution of platina, exposed for about two days in a receiver of sulphuretted hydrogen gas over mercury, absorbed more than ten cubical inches of the gas. I found that a cubical inch of the same solution of platina required about eight cubical inches of hydrogen to reduce the platina to the metallic state.

of the metallic sulphurets in union with sulphuretted hydrogen. This novel field of inquiry has hitherto been neglected; but it cannot be unworthy the attention of chemists, and it promises to lead to the explanation of many difficult or obscure problems in chemistry.

Since the publication of my paper "On the Combinations of Sulphur and Phosphorus with Platina," I have observed a slight inadvertency I had committed, in the use of the words *super* and *sub*. In conformity with the language of chemistry, those compounds should have been called sulphuret and super-sulphuret; and phosphoret and super-phosphoret. In prefixing the words *super* and *sub* to the common names, sulphuret, phosphoret, &c. in any instance, it seems evidently to be implied that there are three distinct combinations of the kind; and in a relative sense, one is said to contain a deficiency, the other an excess, and the third, an intermediate proportion of the inflammable principle. What however, was at first used by accident and erroneously, must now be adopted from design, in the case of the compounds of platina with sulphur, if the experiments detailed in the preceding pages are correct.

I have already stated that the substance obtained by heating the hydrosulphuret of platina to redness in close vessels, is analogous in its physical and chemical properties to the super-sulphuret of platina. Thus its particles are loosely coherent, it is tasteless, it gives lustre to the fingers or paper like black-lead. It is unaffected by air and water, is insoluble in the mineral acids, and is decompounded by the agency of heat and air, with similar phænomena to the super-sulphuret. It differs however in some respects from this substance. Thus its colour is blueish black, and the proportion of sulphur it contains, does not exceed 22 per cent.

In two experiments in which five grains of this substance were decomposed at a red heat in a platina crucible, 3·9 grains of platina were obtained.

And 5 : 3·9 : : 100 : 78.

In a third experiment, nine grains of it afforded rather more than seven grains of platina. Hence it may be presumed that 100 grains of this substance contain

Platina	78
Sulphur	22
	100

From a comparison of my former experiments with these,

these, it seems that there are three distinct combinations of platina with sulphur. The sub-sulphuret appears to contain one proportion of sulphur, the super-sulphuret two proportions, and the sulphuret one proportion and a half. The results which would be furnished from calculations of this kind, do not differ materially from those actually obtained.

The quantity of sulphur which combines with 100 grains of platina in these several compounds I have here annexed.

Grs. of Platina.	Sulphur.	
100	19·04	Sub-sulphuret *
	28·21	Sulphuret
	38·80	Super-sulphuret.

[To be continued.]

XL. *Description of a Machine for pumping Water used in the East. By M.* CHASTELLAN, *Author of* "Travels in the Morea †."

THE art of irrigation ought to be better understood in countries which are subject to drought, than in those where abundant and periodical rains seem to render irrigation less necessary: there are circumstances nevertheless under which it is desirable, even in European countries, to procure an abundant and judicious distribution of water: and the expense of a machine with the necessary reservoir for this purpose will be speedily recompensed.

Watering by hand-engines is frequently disadvantageous, and always tedious. If the gardener has a large piece of ground to manage, he must necessarily water it during the heat of the day, for he cannot have sufficient time before sunrise or after sunset. Besides, the water which he employs for this purpose being newly drawn from a well, or having been contained in deep reservoirs, it cannot get rid of its too great coolness, or of that crudity, which are so injurious to plants; whereas the same water contained in large reservoirs and in uncovered canals for irrigation, soon acquires the temperature of the atmosphere, and the properties of rain water. The way in which the water

* I am aware that the nomenclature here adopted is defective, and founded on an erroneous principle; that there is an excess, a deficiency, a neutral point in these combinations It gives no information concerning the proportions of the constituents of the compounds. Professor Berzelius, I believe, intends shortly to publish some Observations on the Chemical Nomenclature, in which these deficiencies, I have no doubt, will be happily supplied.

† *Bib. Phys. Econ.* Dec. 1811.

water is distributed in the kitchen gardens of Constantinople, completely secures these objects.

The well is generally dug in the highest part of the ground; and the reservoir being still higher, it is capable of furnishing the water necessary for feeding the canals which are to distribute it through the garden. From this reservoir a canal is made, which is afterwards subdivided into gutters cut on the edges of the principal walks: these gutters are again intersected at right angles by others which run between the beds where the various plants grow.

In working these beds, care is taken to throw up a little earth on the edges, so as to form a dyke several inches in height, and solid enough to resist the entrance of the water which is above the beds in point of level: when the reservoirs, the canals, and the gutters are filled with water, the gardener with his spade makes an opening in this dyke, or rather removes a small piece of stone or wood which forms a kind of sluice-gate: the water is then precipitated with rapidity from the canal into the bed, and flows over the whole surface. If one watering is sufficient, he shuts up the sluice and passes on to the next bed, and so on, until he waters the whole garden, varying the irrigation according to the nature of the plants. The same process is applicable to fruit-trees also.

It will not be foreign to my purpose, if I here explain the method of forming the large reservoirs just mentioned. Their construction is simple and œconomical. It consists of planks dove-tailed together, and fastened at the angles by iron bands. This caisson, which may be compared to the tubs used for orange trees, is like them insulated from the ground by four, six, or eight feet, to prevent the wood from rotting by being in contact with the earth. The deals of which it is composed are tarred on each side, and sometimes it is lined with tarpaulin.

As to the machine which raises the water to the height of the reservoir, the mechanism is scarcely more complicated than that of a common draw-well, and indeed it does not differ in an essential degree except in the form and composition of the buckets, which are in the form of funnels or conical bags made of leather, and which empty and fill themselves by turns, thus furnishing abundance of water.

We use in general wooden buckets, which discharge the water into the reservoir by means of a handle attached to them, which gives them a see-saw motion. This mechanism, so simple in other respects, is attended with the disadvantage of being soon worn out: the continual friction and

and agitation shake the buckets and even the reservoir, and speedily destroying both. The wooden buckets in common use, being armed with iron hoops, have the same disadvantage: they are apt to lock against each other in ascending and descending; and by striking continually against the sides of the well, they derange them in the long run. By using leather buckets all apprehensions of this kind will be removed; for the emptying and filling of their contents is performed in a regular manner, almost without friction, and without any kind of shaking; and although leather may appear less durable than wood, it is in fact more so when it is continually soaked in water: it then beeomes extremely elastic, accommodates itself to every kind of motion, and receives a blow from any other body without being injured.

It is probable that the ancients made use of leathern buckets for pumping up water. At any rate, the origin of the employment of leathern vessels, applied to the mechanism of wells, must have been derived from the primitive use of that kind of funnel which served formerly, as it now serves, to carry liquids.

In the suite of the Turkish armies there are generally a great number of led horses, mules, or camels, which have no other business than that of carrying water from the rivers to the field. The buckets in which it is conveyed are of a conical form, the base being closed by a flat piece of wood. They are buckled on each side of the pummel of a saddle, and the other extremity, tied by a cord to prevent the water from escaping, is folded up, and also attached to the saddle, along the flanks of the horse, which are guarded from moisture by a leather covering. This form of the buckets, while it holds abundance of water, is not troublesome to the horse, which may be ridden at the same time with ease. It is only necessary to unbuckle them from the saddle when they are to be filled; for, in order to distribute the water afterwards, all that is requisite is to untie and lower the small extremity.

In order to use these conical buckets to draw water from a well, a cistern, or even from a river, the two extremities are to be kept open by means of iron hoops to which cords are attached.

In order to pour water into a reservoir from them, an extremely simple mechanism of the following form is employed.

Explanation of Plates IV. *and* V.

A kind of triangle is formed over the well, the beams tied together

together at top, and sunk into the earth at proper distances below.

Fig. 1. presents the elevation, and fig. 2. the section of the machine.

At the summit of the triangle is fixed a pulley *a*; lower down, and nearly as high as the reservoir *b*, there is a pulley or wheel *c* which turns upon pivots. The cord *d*, which is attached to the large aperture of the bucket *e*, passes into the pulley *a*; another cord *f*, fixed to the extremity of this funnel *e*, passes over the wheel *c*, and both are fastened to a windlass.

If we suppose the bucket *e* at the bottom of the well, it will be filled with water; and when the windlass is turned and the cords are stretched, it begins to ascend; the small end of the bucket *e* is folded up; in this position the water which it contains cannot escape, this extremity being as high as the large aperture. By always drawing at once the two cords *d* and *f*, the bucket *e* will preserve the same position, until it rises to the edge of the well; but when it arrives to the height of the reservoir, the small extremity passes above the wheel *c*, the other ascends to the pulley *a*, the bucket *e* is unfolded, assumes the real form of a funnel, the office of which it performs, and the water finally escapes by the lower aperture, and falls into the reservoir *b*. The windlass immediately goes round; the bucket *e*, resuming its first position, redescends by its proper weight to the bottom of the well, where it is again filled: the windlass turns round again, and makes the bucket ascend a second time, and so on. It will be seen from this, that in a little time the reservoir *b* is filled, and afterwards pours the water by a gutter into a lower basin, from which it may be distributed over the whole garden.

It ought to be nevertheless remarked, that there is some time lost in the return of the windlass; but this return may be made useful, by placing the windlass across the middle of the well, but so that the handle *g* may come to one side, between two machines: then we shall have two reservoirs *b b*; each of the cords *f f*, attached to the small end of each bucket *e e*, after having passed over the pulley *c c*, will traverse the reservoir *b b*, pass under another parallel pulley *i i*, and will return to unite with each of the cords in the pulley *a a*: the four cords will be fixed by their extremity to the windlass, so that, by being unrolled and developed successively, one of the buckets will be filled at the bottom of the well, while the other will empty itself into the reservoir.

To

To convey a clearer idea of the working of the buckets, the two pulleys *a a* have been separated, and the aperture of the well widened; but they may be joined in a parallel manner on one and the same pin or pivot.

To conclude: This mechanism may be modified according to circumstances. If the well be situated in low ground, the reservoir must be raised to a height sufficient to command the whole garden.

On the banks of a river, it will be sufficient to drive two piles into the current: they will support the extremity of two horizontal beams, the other extremity of which will rest on the bank. The whole mechanism will be established on these two powers, and the reservoir situated on the banks will be raised to the height desired, in order that the water may be carried to a distance by means of wooden troughs or gutters.

The mechanism exhibited in fig. 2. seems best calculated to lessen manual labour in procuring a supply of water by the machine in question. The windlass is a common carriage wheel, furnished with a handle. But in order to obtain a still more certain and powerful result, we might furnish the extremity of the roller *i* with a wheel and pinion (fig. 3): this very simple mechanism would enable a child to work the machine.

As to the disposition of the cords, pulleys, and rollers, an inspection of the plates will suffice. We would recommend, however, to place the pulley at such a height, that the bucket may be perfectly developed on rising to the height of the reservoir; we ought to attach the cords to the two extremities of the roller which serves for the windlass, and, if it is possible, proportion the length of this roller to that of the cords which it ought to receive; for it would be desirable that they should not make several revolutions upon themselves; which nevertheless can scarcely be avoided when the well is very deep.

As to the proportion of each bucket, and its capacity, it must vary according to the quantity of water wanted; but in order to avoid the trouble of calculation to our readers, we shall give an approximate table.

Every bucket represents a truncated cone.

A cone five feet in height, on a base twenty inches in diameter, will contain 3,58 cubic feet of water, or nine pailfuls and nine tenths *.

* We have here valued the dimensions of a pail at nine inches in diameter by six inches in height, and consequently its capacity at 0,36 of a cubic foot.

A cone

A cone four feet by sixteen inches will contain 1,84 of a cubic foot, or three pailfuls three-tenths.

We ought to make an abstraction of the loss of water occasioned by the fold which the cone contracts in ascending from the bottom of the well, since it is suspended by the two extremities; this loss may extend to half a pailful, and we have also neglected the capacity of the truncated portion of this cone.

The largest of these buckets cannot be easily managed but by means of a windlass furnished with a hopper wheel; but the smallest may be managed by a simple windlass with a handle.

The bucket is the most important part of this machine, and it is even in this that the innovation consists; for the rest of the mechanism may be varied *ad infinitum*, and we may apply the same kind of bucket to all the machines in use for drawing water. It will procure in the same space of time, and with the same force, a volume of water much more considerable; and it combines another great advantage, that of conveying the water to any given height by means of one or more machines of this kind. For this purpose, it is only necessary to raise the reservoir parallel to the level of the greatest height of a sloping piece of ground, and to cause the water to flow from the reservoir into channels formed by hollow trees, or simply by two planks nailed at right angles, and tarred at the joinings.

Supposing that we only wish to reach a perpendicular height of twelve feet, we shall elevate the first reservoir on four posts to this height. We may conceive that by carrying the pulley some feet above the reservoir, the bucket will ascend, and will thus be equally well emptied at this height as at the level of the ground, although the mechanism necessary is placed below this same reservoir. We shall even find an advantage in this; for the gardener will be protected from the sun, and the four posts may be concealed by trellis work, to which we may apply vines or other creeping plants or flowers, which will give the scaffolding the appearance of a harbour.

Finally, the mechanism just described may be applied to the external wall of a house; and if we form a reservoir on the roof, the water may be conveyed throughout the whole of the apartments, and serve several useful purposes.

 XLI. *On*

XLI. *On the Substitution of the Oxymuriate of Magnesia for that of Lime, in the Process of Bleaching, &c.* By M. Donovan.

To Mr. Tilloch.

Sir, The readers of your Magazine are already in possession of the facts stated by Dr. Ogilby to the Kirwanian Society, relating to the effects of oxymuriate of lime in bleaching. I was present at the reading of the paper, and with many others examined the specimens of linen which had suffered immersion in the solutions of muriate of lime. These specimens, as far as I could judge, appeared to be perfectly sound: the rest of the members also declared themselves satisfied that the linen had not been perceptibly injured.

Sir H. Davy, in his lectures delivered in Dublin 1810, observed that he had sufficient reason to believe that muriate of lime does a material injury to the linen fabric. In the "Elements of Chemical Philosophy," Sir H. states that linen boiled in a strong solution of muriate of lime is "considerably weakened." Lately, also, it has been positively asserted that the injury done by muriate of lime amounts to absolute rottenness. On the other hand, Dr. Ogilby and some gentlemen who have repeated these experiments still maintain that the linen in this operation suffers nothing.

Somewhat amazed at such extraordinary and unqualified contradictions, I was resolved, merely for my own satisfaction, to attempt an experiment on the subject. My results I communicated to Dr. Ogilby; and at his instance, as there had been some difference in our methods, I ventured to make public whatever I had ascertained.

I first prepared a perfectly neutral muriate of lime: the solution was of such a strength, that it remained fluid during the day-time only, but as the coolness of evening advanced it deposited a few crystals. In this saturated solution I immersed a piece of sound linen, and suffered it to remain for 48 hours; at the end of which period I washed and dried it; but found it, as well as I could judge, as sound as before.

I then took another piece of the same linen, and boiled it for an hour in a new portion of the same concentrated solution of muriate of lime. At the end of this period it was allowed to cool; the muriate had become a solid white mass, of considerable hardness. As much water was poured on on as seemed sufficient to obtain a solution as saturated as at first; and in this state the whole was left for 24 hours. The specimen after washing and drying was not perceptibly injured.

Sensible, however, that I could not possibly be a competent judge of the strength or soundness of linens, I resolved to submit the matter to persons who, from constant experience, should be supposed adequate to the task of deciding the question. I accordingly handed the specimens to some persons of great respectability who are engaged in the linen trade, and whose names if necessary shall be made public. These gentlemen permit me to state, that in their opinions the linens were perfectly sound and merchantable.

In the preceding experiments, it is to be observed, that the solutions of muriate of lime were considerably more concentrated than any that are used by the bleacher; and I thought it probable that if the muriate ever exerted any deleterious influence on the linen, it would be the more considerable in proportion as the power of the deleterious fluid was increased. If this be fact, and we can scarcely doubt that it is, the second trial was particularly calculated to effect the destruction of the cloth; for the muriate was in a state of fusion, and consequently was brought to the utmost state of concentration that could be employed in experiment.

The foregoing is a simple statement of facts as I found them. I am much at a loss to account for the difference between my results and those of others. I will not say that Sir H. Davy has been deceived; it is just as likely that I have been deceived: yet it will be allowed, should the latter prove to be the truth, that there must be something in the matter with which we are as yet unacquainted, and which consequently requires elucidation.

As well as I can recollect, Sir H. Davy, in his lectures 1810, proposed the oxymuriate of magnesia as a substitute for the oxymuriate of lime in all the processes of bleaching. I am the more satisfied in this opinion by the coincident testimony of others: indeed the matter appears to be demonstrable from certain passages in Sir H. Davy's Elements, and from his observations on the destructive influence of muriate of lime on linen.

The impracticability of this proposal is, in another part of this Magazine, adverted to by Dr. Ogilby, on account of the enormous comparative expense of magnesia. What compensating advantages are to be derived from the substitution?

tution? None are even offered, except the harmless nature of the resulting muriate of magnesia. Muriate of lime is declared to be injurious, on the evidence of experiments which do not succeed in the hands of others: and the bleachers deny the fact altogether. It is certain that many pieces are found to be damaged by bleaching with the oxymuriate of lime; but this is universally known to be produced by negligence or ignorance, and not by the causticity of the resulting muriate.

But although, in Sir H. Davy's experiment, the "very strong solution of muriate of lime" did "considerably weaken" the linen, how is the fact connected with the process of the bleacher, who never makes use of any other than weak solutions?—Certainly not at all, unless a mode of reasoning be introduced which has been attributed to Sir H. Davy, namely, that "if a strong solution act energetically, a weak one must act proportionately so:" but as this kind of argument has been disclaimed, I am far from insisting on the matter: hence the above question remains to be answered.

Now although Sir H. Davy does not appear to have been aware of it, there certainly is an advantage in oxymuriate of magnesia, if that substance could be obtained at even a reasonable price; namely, in bleaching calico destined for printing, before it comes into the hands of the printer. After the immersion of the calico, according to the usual process, in oxymuriate of lime, the workmen are sometimes so negligent as not perfectly to wash out the muriate; so that, during the souring, a sulphate of lime is precipitated into the substance of the cloth, which cannot be removed without more washing and trouble, than usually is, or can well be, bestowed on it. This sulphate acts as a mordant to certain colours used in the subsequent process of printing, and occasions a dulness of those parts which should be of a clear white; and perhaps makes the delicate shades of colour somewhat different from what was intended.

It is no wonder that negligence in this, as well as in every other case, should be productive of inconvenience: when the workmen do their duty, nothing disagreeable can result from the abovementioned causes.

With a view of guarding against negligence, the oxymuriate of magnesia certainly possesses advantages; for even should the resulting muriate be not perfectly washed out, the sulphate formed during immersion in the sours is so soluble

luble a salt that it will readily give way to a few washings; and were a portion to remain, it would not act as a mordant. The substitution on this account were rational, and I believe the merit of it belongs exclusively to Mr. Duffy of Dublin; for it will be allowed that the views of Sir H. Davy were completely of a different nature.

From these advantages, however, the bleachers are unhappily excluded, on account of the enormous price of magnesia. Sir H. Davy observes in his Elements, that this earth may be "easily procured from sea-water, or from the residual liquor of salt works." The fact is indisputably true, and so well known, that the manufacturers obtain it in this, and, I believe, no other way. Hence, I fear, the obstacle of its price is not the more surmountable from the information conveyed in the above passage; at the same time, not doubting that had Sir H. Davy turned his attention more minutely to the subject, his usual ingenuity would not forsake him.

In the Elements of Chemical Philosophy, we read, that at the suggestion of Sir H. Davy, the oxymuriate of magnesia had been used by Mr. Duffy "*within the last few months*" for whitening printed calicos. It is with great hesitation I say, that there must have been some misunderstanding between these gentlemen; either party must certainly have misconceived the other's meaning: and this is not asserted without some foundation in proof. Mr. Duffy informs me that he had used the substance in question, for experiment, as early as December 1810, and largely *in the beginning of* 1811; a period much more distant than "the last few months."

Mr. Duffy, far from taking any merit to himself, for the application of oxymuriate of magnesia to clearing, will not even allow any to be attributed to him; he is anxious that the credit of the matter should lie with the person who *first* conceived the probable success of the process, without taking upon him to decide who the person may be. I shall therefore further the views of Mr. Duffy, and state all I know on the subject.

When that part of Dr. Brewster's Edinburgh Encyclopædia appeared, which contained the article "Bleaching," I was much amazed and displeased at finding that the writer of that article had assumed to himself the merit of applying the oxymuriate of magnesia, without once having mentioned the name of Sir H. Davy. Anxious to ascertain who had the real title, I made an inquiry in Scotland;

the result of which was, that the idea was originally conceived by Mr. William Ramsay of Glasgow, that it is about four years since he applied it to the clearing of printed calico, in the way of experiment; but that it is three years since he made known the manner of preparing the salt to a calico printer, Mr. T. Arthur at Levenbank, in the county of Dumbarton, who *employed it very extensively;* and that Mr. Ramsay also communicated it to a bleacher at Lennox Miln printfield. Some time after this, the house in which Mr. Ramsay is concerned received a letter from Belfast, offering for sale the very process which he himself had originally communicated. It was fortunate enough for Mr. Ramsay that he had not kept his secret entirely to himself, as otherwise he might now perhaps be unable to lay such irresistible claims to originality.

I also wished to know from the bleachers of Scotland, what was their general opinion concerning the supposed deleterious influence of muriate of lime. I was not displeased at finding, that in the use of this substance there was not one disagreeable circumstance that could lessen the merit, or subtract from the importance, of Mr. Tennant's most valuable discovery.

It now only remains to notice the precise situation in which the matter rests: Sir H. Davy at first proposed the substance in question, as a general agent in bleaching. The substitution appears to have been grounded on an assertion at present denied upon all sides; and the proposal itself is said to be impracticable from its expense. Sir H. Davy, by his own account, proposed his process for whitening only within the last few months; but Mr. Ramsay had put it to practice almost four years before.

Such are the results of my few inquiries on the subject. Should any be inclined to dispute the claims of Mr. Ramsay, I have little doubt that all the parties concerned, with perhaps several others, will, if called upon, give their united testimony in favour of the above statement.

I am sir,

With respect, yours, &c.

M. Donovan.

Dublin,
Sept. 21, 1812.

XLII. *Pro-*

XLII. *Proceedings of Learned Societies.*

LONDON PHILOSOPHICAL SOCIETY.

[Continued from p. 154.]

Mr. Reid then proceeded to a review of the Roman stage, deriving its origin from a similar source to the Greek, the Saturnalian games. But though Livy and others bear witness, he said, to the extravagant love which that people had to scenic exhibitions, the works of the two Senecas are the only considerable specimens of their tragic Muse that now exist. Livius Andronicus, the next ancient and least elegant of all their dramatic writers, was for some time the only star visible in the dramatic horizon, till Nævius rose to rival, or rather eclipse him.

Ennius followed, who was a well-known favourite of the immortal Cicero, and of whom Lucretius says, that he was the first of their poets deserving of a lasting crown from the Muses. The Romans were the first to throw off the shackles of the chorus, and Plautus had the merit of the innovation. The comedies of this writer abound with humour, but it is a humour of a coarse and barbarous kind. In the midst however of low buffoonery, his characters have the merit of strong discrimination. Terence, treading in the footmarks of Menander, achieved the perfection of the Roman drama; but a tiresome want of variety exists in his characters; which may be traced, however, more to the secluded and insipid loves of the Roman females, than to the deficiency of the dramatists, whose polished and elegant style, adorned with a charming and delicate simplicity, amply atones for the defect.

The peculiarities which distinguished the ancient stage from the modern next claimed the attention of the lecturer; and after justly observing, that the modern introduction of acts superseded the necessity of that strict observance of the unities for which some critics have been such sticklers, he remarked that Æschylus was the first who introduced masks upon the stage; previous to which time the actors merely concealed their faces from the spectators. The different age, sex, and character were strongly depicted on these masks; and it is a remarkable fact, strongly illustrative of ancient devotion to physiognomy, that it was thought essential for every character to be marked by a particular quiescent cast of countenance; and that whenever a play was delivered to the actors, the author gave drawings of the

 masks

masks adapted for it; and if a variety of passions were included in the same character, the actor had either different masks, or a different passion painted on each side, which was displayed alternately in profile to the spectators. The delicate representations of the mind expressed in the physiognomy were therefore entirely forsaken by the ancients; but it was a loss which they could not appreciate, as the extent of their theatres would have rendered such representations impalpable to the sight.

After remarking on the acoustic properties of the ancient masks, and the violent gesticulations of the Roman actors, Mr. R. asserted, that a taste for gesture began, and at last a total contempt of every thing but show completed, the degradation of the Roman stage, the decline of which may be dated, as well as of every species of literature, from the establishment of legal tyranny by Augustus. The decline of that empire led the lecturer, by easy transition, to the state of the drama during the dark ages, to its subsequent revival in Italy, Germany, France, and England, and its latter purification. The improvisatori of Italy were the first revivers of the stage. These performances, which were characterized by little but buffoonery, were succeeded by religious representations; and the *new* birth as well as the *first* birth of the drama was marked not only by the same features, but by the same name,—that of mysteries. It is curious to remark, that the English stage resembled for some time the Eleusinian representations; for it personified the virtues, the vices, and the moral propensities, as well as the sainted demigods of the new religion. Nor, much as we may doubt the amusement resulting from it, can we suffer severity to conquer our feelings of indulgence, when we recollect that it produced the Paradise Lost of Milton (originally a dramatic mystery), and most probably suggested the Fairy Queen of Spenser. Metastasio in Italy was the great champion of the ancient stage, and the mysteries yielded to the purer taste of his tragedies. Operas, said the learned lecturer, are the peculiar invention of the Italians, and they owe their birth and their success to the harmony and music of their language. Disagreeing with Addison, therefore, in his general severity against the opera, he yet was of opinion that in this country it was an exotic, fostered and kept alive by a ridiculous fashion, where the taste, the language, and the manners of the people were in every respect unpropitious.

From Italy Mr. R. passed to Germany; and after remarking the singular fact, that the drama of that country was

was but a century ago little advanced from the rude buffoonery of the dark ages, and the superstitious mummery of the mysteries, an accidental circumstance, he said, reformed it. A company of French actors introduced the works of Corneille; and the productions of Schiller and Kotzebue were the almost immediate consequence of the emulation they inspired. In appreciating the characters of these writers, he inclined to think that the plays of the former were not only much less unexceptionable than the latter, but were calculated, by exciting a generous sympathy, to refine and moralize mankind: but, with regard to Kotzebue, he stigmatized his dramas as in the last degree immoral, and pernicious to the well-being of society *.

The French stage, said Mr. R. famed as it is for chasteness and morality, was unworthy of notice till the time of Moliere and Corneille; and after drawing a just comparison between the characteristics of these two dramatists and those of Racine and Voltaire, he proceeded to point out the disadvantages under which the French stage laboured, from the necessity of adapting rhyme instead of blank verse to its loftier dialogue; observing, however, that the regularity of conduct, the regard of unity, and the chastity which characterize it, compensate in a great degree for what to English ears is a very great defect.

Minstrels introduced the stage into England as well as into Italy. About thirty-six years after the Conquest, a man of the name of Rahere amassed a considerable property by interludes; and this first suggested to the clergy the performance of mysteries; so that the new priesthood made use of the same instruments for effecting the same purposes as the old, that is, to acquire a total influence over the minds and morals of the people, by speaking to the eyes and to the senses. The love of these performances became so great in time, that it was found necessary to suppress them in 1258, by an injunction from the Barons. From this time to Henry the Eighth, a stupor seemed to have invaded the dramatic muse, when Heywood, the king's jester, introduced little dramas in which the characters and manners of people in low life were represented;—an effort which excited such emulation, that the whole country abounded with players; in consequence of which another suppression took place at the time of Elizabeth.

The lecturer, proceeding to a consideration of the genius of Shakspeare, agreed with Dryden in the remark that he

* The asperity of this observation, as also that respecting the opera, was in the course of the discussion much softened,—the latter indeed combated.

found

found not, but created the English stage. Massinger, Ben Jonson, Ford, Webster, Beaumont, and Fletcher, came alternately under his investigation; and after a successful analysis of their different styles, he reviewed the licentiousness of taste which disgraced the reign of Charles the Second, and, comparing it with the cast of modern plays, drew a very just conclusion as to the general increase of public morality. During the time of Charles there were no less than seventeen play-houses in London, all of which were put down by the Puritans.

The appetite for theatrical amusements, which succeeded the overthrow of fanaticism, may be accounted for by the long fast which the people had sustained. The theatre, however, arose under considerable advantages. Patents were granted to Killegrew and Davenant, incorporating a body of actors for the amusement of the public; and the introduction of women on the stage, whose parts had till then been personated by boys, was a change of great improvement to the drama.

After some just remarks, deduced from the display of character by females, on the illiberality of inferring an inferiority of faculty from defect of cultivation, the characteristics of Otway, Cibber, Vanbrugh, and Congreve produced from the lecturer an instructive analysis of their peculiar qualifications. The libertinism of the two latter drew upon the theatre the well-directed attack of Jeremy Collyer, who succeeded for a time in mastering the witty arguments of his opponents supported by Dryden; and the stage had scarcely recovered from the shock, when the ingenious and celebrated Henry Fielding, by the personality and sarcasm of his dramatic innovation, produced a severer enemy in the person of ministers; the consequence of which was, that the power of licensing more theatres as well as all plays was vested in the Crown. Foote a short time after followed the same plan; but, having secured a strong party in his favour, overruled the opposition the revival excited. Mr. R. however, drew a very proper distinction between the personalities of Aristophanes and those of Foote and Fielding, who have been compared to him, observing, that virtue was an object of attack to the first, but immorality and folly *alone* provoked the satire of the latter. After a just and appropriate compliment to the talents of Garrick, the author passed to the consideration of the new species of sentimental comedy introduced of late years on the stage. Diderot, who invented it, merely called it *la drame*; but the enemies of his project nicknamed it, in ridicule of its sentiment,

sentiment, *la comédie larmoyante.* The mention of the innovation introduced a critique on the English pupils of this style, and Colman, Cumberland, Morton, and Tobin, received the tribu e of praise which they deserved.

Mr. R. after discoursing on the extensive influence and moral effects of the drama, and arguing that latter years had refined it in every point of view, deduced, that all the arguments of any weight against the drama are really arguments merely applicable to the œconomy of the theatres; and he pursued the subject by suggesting hints for the improvement of this œconomy, proposing in the first place an increase of theatres, which, by reducing their size, would strike at the root of unmeaning spectacles and shows; and secondly, in the exclusion of all company that offend the eyes and ears of respectable females.

Some accurate observations on the pleasures resulting from illusion in the drama, and on the Godwinian theory, that plays will in time be nugatory and useless, as man invigorated himself in intellect and purified himself in morals, concluded the excellent discourse.

The same month, Mr. T. Pettigrew delivered a lecture on Zoology, which embraced the order *Primates* of the class *Mammalia*, having before, in an introductory lecture to a course intended by him to be given to the Society, taken a general view of the different systems of classification of the animal kingdom, and of the immensity of nature observable in that grand division of it.

The lecture was rendered exceedingly interesting by a well chosen selection of anecdotes, illustrative first of their wild, and secondly of their domesticated state, and an exhibition of very excellent specimens of most of the species which came under the lecturer's consideration.

We forbear entering into a description of the animals given, as a detail of specific character would be tedious, and without the specimens uninteresting: but we cannot but highly approve of the Society's endeavouring to diffuse a general spirit of inquiry in the various branches of science. The manner likewise of treating it by anecdote, relieves the study from that dryness and fatigue which have been said particularly to characterize natural history.

XLIII. *In-*

XLIII. *Intelligence and Miscellaneous Articles.*

On the Conversion of Starch into Sugar.

To Mr. Tilloch.

SIR, YOU will much oblige me by correcting a mis-statement in my communication published in your Journal for August, "On the Production of Sugar from the Starch of Wheat and of Potatoes, by the Agency of Sulphuric Acid."

In that paper I stated that the discovery had been communicated to Professor Berzelius by M. Kirchoff of St. Petersburgh; but I have since been informed by the Professor, that he did not receive the communication *directly* from that gentleman. I beg that you will excuse this trouble, and am, sir,

Your humble servant,

London, Sept. 21, 1812. WM. MOORE.

To Mr. Tilloch.

SIR,—ALLOW me to request from you, or from some of the scientific readers of the Philosophical Magazine, an answer to the following query.

Every one knows that muriat of silver, on exposure to the sun's light, becomes in a short time of a dark hue. Is there any material, solid or fluid, simple or compound, which being of a dark hue will, on exposure to the sun's light, become *in a short time* lighter in its tint, if not wholly colourless?

I am, sir, yours, &c.

R. Y. S.

SIR HUMPHRY DAVY AND PROFESSOR LESLIE.

To Mr. Tilloch.

SIR,—IN glancing over Davy's Elements of Chemical Philosophy, I was surprised to find it alleged, that Van Helmont had given a sketch of a "curious instrument, very similar to the differential thermometer;" and a few pages further on, to meet with this bottom note—"Plate I. fig. 2. represents Mr. Leslie's differential thermometer." [See Plate V. fig. 5.] "Fig. 3. is copied from Van Helmont." [Plate V. fig. 6.] "This instrument appears to have been the first in which the expansive power of heated air was exhibited by its action upon cold air." On turning

to

to this plate, I observed a very clumsy and distorted representation of my differential thermometer, placed by the side of another figure, bearing a general resemblance to it, and purporting to be a copy of Van Helmont's sketch. Anticipations, such as are here insinuated, have no doubt occurred in the history of scientific discovery; but there has been so strong a propensity of late to bring forward allegations of this nature on the slightest grounds, that I was tempted, for the first time, to examine the original work. I soon perceived, however, that Van Helmont's description and figure were essentially different from the representation which Sir Humphry has taken the trouble to give. In fact, the "curious instrument" described by the alchemist is no other than the common air-thermometer of Sanctorio or Drebbel, invented more than forty years before his death; only, for the sake of easier carriage, shaped like a syphon, the lower end being bent upwards, and terminating in a spherical cup, with a *small orifice*—one of the forms which it had from its earliest introduction. A learned person of the name of Heer imagining, it seems, as Sir Humphry has since done, that the instrument was absolutely closed, had proceeded to admire the *perpetual motion* of the contained liquid, and next to wonder at its gradual disappearance, which he sagely regarded as an irrefragable proof of the conversion of water into air. To this Van Helmont replied, that the action of the machine no more produced perpetual motion than the changing of a weather-cock; and urged that, if both the balls were shut, the liquor must, under all the changes of external heat, remain stationary, being pressed by the equal and opposite elasticities of the secluded masses of air—a statement which incidentally involved the principle of the differential thermometer, but which the author never once dreamed of reducing to use. On the contrary, he calls his rival, in the uncourtly language of those times, "an idiot," and charges him with "stupidity," for not perceiving that the instrument had an *aperture*, only slightly shut with a stopper, and not hermetically sealed, as it is most incorrectly figured in the "Elements of Chemical Philosophy."

I will not suppose that Sir Humphry intentionally misrepresented the meaning of Van Helmont; but then it follows, that either he had not read the passage to which he refers, or must have satisfied himself with a very superficial and careless inspection. This precipitancy is the more to be lamented, as it may possibly beget suspicion of want of of accuracy or fairness in other matters of higher consequence.

I am, sir,

Your most obedient servant,

Edinburgh, Sept. 6, 1812. JOHN LESLIE.

P.S.—I inclose a copy of Van Helmont's sketch. [See Plate V. fig. 7.]

ANTIQUITIES.

The tessellated pavement discovered last year at Bignor in Sussex, was covered with earth to preserve it during the winter. It has been lately opened again, and the surrounding land dug up, for the purpose of further discovery. A series of apartments are now exposed, all paved with beautiful mosaic, the most of it in the highest state of preservation, and exhibiting perhaps the best specimen of the kind in this country. The various figures are well defined and delineated, some of them very beautiful, particularly an eagle with Ganymede, a pheasant, a dolphin and some others. Walls are erecting on the ancient foundations, the ruins furnishing materials, so that the plan of the building may be tolerably traced. It no doubt has been the villa of some of the Roman generals, the chief city of the Regni, Chichester, where Vespasian fixed his head quarters, being within a few miles, and the ancient Roman road thence to London crossing the South Downs directly in front of the edifice. The surrounding scenery is very romantic, and must have been always interesting. We conceive the destruction may be dated, with that of many other monuments of the power and splendour of the Romans at one time in this county, from the barbarous invasion of the Saxons under the ferocious Ella, who, irritated with the formidable opposition he met at Chichester, ravaged it and the surrounding country with fire and sword with the most unrelenting fury. So completely had time effaced all appearance of former habitation, that the same family have ploughed the field every year for thirty years past, without the remotest suspicion of the treasure it contained, till last autumn the ploughshare came in contact with one of the large stones of the building.

EXTRAORDINARY POWER OF IMAGINATION.

"During our stay at the Dardanelles, we lived in the house of the Neapolitan Consul. This respectable old man put in force a stratagem which may serve to show the extra-

extraordinary power of imagination over diseases of the body. Being troubled with an intermitting fever, brought on during our excursion in Troas, I had been observed by him to go frequently to a clock in the anti-chamber of our apartment, watching for the hour when the paroxysm began. This used to occur exactly at noon. One morning he put back the clock a full hour. At twelve, therefore, I had no fear of my fever, for the index pointed to eleven; and at one, although the hour seemed to be present, the paroxysm did not take place. Unfortunately, pleased by the success of his experiment, he told me what had happened; and after the usual interval the fever again returned. By the same manner, all the charms used among the lower order of people in this country operate in the cure of agues. The Tomb of Protesilaus, as related by Philostratus [in Heroïcis. — See also Chandler's Ilium, p. 142], was anciently resorted to in healing a quartan fever." —*Clarke's Travels,* part ii. sect. i. p. 173.

LECTURES.

Dr. Adams's Autumnal Course of Lectures on the Institutes and Practice of Medicine will commence on Thursday the 8th of October, at Ten o'clock precisely, at his house No. 17, Hatton Garden, and be continued every Tuesday, Thursday, and Saturday.

Further particulars may be known by inquiring as above, or of the different medical Booksellers, where a Prospectus and Syllabus of the Lectures may be procured.

LIST OF PATENTS FOR NEW INVENTIONS.

To Roger Thompson, of North Shields, in the county of Northumberland, shipwright, for a new mode of working two or more pumps for delivering water out of leaky ships, stone quarries, or mines, of a moderate depth, employing in the operation only about half the usual manual force, and delivering nearly double the quantity of water.—5th August, 1812.

To Thomas Hubball, of Clerkenwell Close, in the county of Middlesex, japanner, and William Robert Wale King, of Union Court, Holborn Hill, in the city of London, tin plate worker, for their new and improved method of ornamenting articles, japanned, painted, or sized, whether made of paper, wood, or any metallic substance; as also leather, oil cloths for tables or floors, and wainscot or plaster walls or partitions.—6th August.

METEORO-

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For September 1812.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
August 27	57	67°	53°	29·82	65	Fair
28	52	59	52	·95	32	Fair
29	53	54	52	·90	0	Rain
30	53	57	55	·97	10	Showery
31	55	61	54	30·05	35	Cloudy
Sept. 1	55	60	53	·12	46	Cloudy
2	54	66	56	·15	61	Fair
3	56	64	55	29·92	47	Fair
4	57	68	54	·84	56	Fair
5	55	65	56	·85	52	Fair
6	56	67	55	·90	60	Fair
7	55	68	50	30·02	62	Fair
8	50	70	61	29·96	70	Fair
9	60	66	60	·82	32	Fair
10	61	68	60	·94	40	Showery
11	60	69	56	30·20	59	Fair
12	53	60	53	·25	65	Fair
13	57	70	52	·26	62	Fair
14	50	69	50	·15	60	Fair
15	50	69	51	·08	51	Fair
16	52	70	52	29·90	62	Fair
17	54	61	46	·82	0	Rain
18	47	60	42	30·09	50	Fair
19	45	62	49	·18	70	Fair
20	54	66	54	·10	66	Fair
21	57	72	55	29·98	72	Fair
22	56	66	56	·98	0	Rain
23	55	57	47	30·01	36	Cloudy
24	50	57	42	29·95	30	Cloudy
25	45	62	55	30·10	47	Fair
26	55					

N. B. The Barometer's height is taken at one o'clock.

XLIV. *Observations intended to show that the progressive Motion of Snakes is partly performed by means of the Ribs.* By EVERARD HOME, *Esq. F.R.S.**

ON a former occasion I laid before the Society a description of the mechanism of the hood of the cobra de capello snake of the East Indies, the coluber naja of Linnæus, in which the ribs of the neck are shown to be formed in a particular manner; so that, when they are raised, the skin becomes stretched out, and puts on the appearance of a hood.

The ribs so employed have several peculiarities, which I took for granted were confined to those of the neck, for I was not in possession of the bodies of the snakes, and therefore could not examine the others; but have since found that many of these peculiarities are not only common to all the ribs of this snake, but to those of the whole tribe.

This fact, as it escaped my observation at that time, would have still done so, had it not been for the following circumstances.

A coluber of unusual size, lately brought to London to be exhibited, was shown to Sir Joseph Banks; the animal was lively, and moved along the carpet briskly: while it was doing so, Sir Joseph thought he saw the ribs come forward in succession like the feet of a caterpillar. This remark he immediately communicated to me, and gave me an opportunity of seeing the snake and making my own observations.

The fact was readily established, and I could feel the ribs with my fingers as they were brought forward: when a hand was laid flat under the snake, the ends of the ribs were distinctly felt upon the palm, as the animal passed over it.

This becomes a more interesting discovery, as it constitutes a new species of progressive motion, and one widely different from those already known.

In the draco volans the ribs form the skeleton of the wings, by means of which the animal flies, the five posterior ribs being bent backwards and elongated for that purpose, so that in that instance the progressive motion is performed by the ribs; but those particular ribs are superadded for this purpose, and make no part of the organs of respiration; whereas, in the snake, the ribs are so constructed as to perform their office with respect to the lungs as well as progressive motion.

* From the Philosophical Transactions for 1812, part i.

That ribs are not essential to the breathing of all animals whose lungs are situated in the same manner as in snakes, is proved by the syren having no ribs; but as this animal has also gills, and can breathe in water as well as in air, the lungs are not so constantly employed, and probably a less perfect supply of air to them may suffice.

In animals in general, the ribs are articulated to the back bone by means of a convex surface, which moves upon a slightly concave one formed upon two of the vertebræ, partly on the one and partly on the other, so that there is a rib situated between every two vertebræ of the back; but in the snake tribe, the head of the rib has two slightly concave surfaces which move upon a convex protuberance belonging to each vertebra, so that there is a rib to each of the vertebræ.

One advantage of this peculiarity is, that it prevents the ribs from interfering with the motion of the vertebræ on one another. The vertebræ are articulated together by ball and socket joints (the ball being formed upon the lower end, and the socket on the upper one), and have therefore much more extensive motion than in other animals.

The muscles, which bring the ribs forward, consist of five sets, one from the transverse process of each vertebra to the rib immediately behind it, which rib is attached to the next vertebra. The next set goes from the rib a little way from the spine just beyond where the former terminates; it passes over two ribs, sending a slip to each, and is inserted into the third: there is a slip also connecting it with the next muscle in succession. Under this is the third set, which arises from the posterior side of each rib, passes over two ribs, sending a lateral slip to the next muscle, and is inserted into the third rib behind it.

The fourth set passes from one rib over the next, and is inserted into the second rib.

The fifth set goes from rib to rib.

On the inside of the chest there is a strong set of muscles attached to the anterior surface of the vertebræ, and passing obliquely forwards over four ribs to be inserted into the fifth rib, nearly at the middle part between the two extremities.

From this part of each rib a strong flat muscle comes forward on each side over the viscera, forming the abdominal muscles, and uniting in a beautiful middle tendon, so that the lower half of each rib, which is beyond the origin of this muscle, and which is only laterally connected to it by loose cellular membrane, is external to the belly of the

the animal for the purpose of progressive motion; and that half of each rib next the spine, as far as the lungs extend, is employed in respiration.

At the termination of each rib is a small cartilage in shape corresponding to the rib, only tapering to the point. Those of the opposite ribs have no connection, and, when the ribs are drawn outwards by the muscles, are separated to some distance, and rest through their whole length on the inner surface of the abdominal scuta, to which they are connected by a set of short muscles: they have also a connection with those of the neighbouring ribs by a set of short straight muscles.

These observations apply to snakes in general; but they have been particularly examined in a boa constrictor, three feet nine inches long, preserved in the Hunterian Museum. In all snakes, the ribs are continued to the anus, while the lungs seldom occupy more than one-half of the extent of the cavity covered by the ribs. These lower ribs can only be employed for the purpose of progressive motion, and therefore correspond in that respect with the ribs in the draco volans superadded to form the wings.

The parts of which a description has been attempted, will be better understood by an inspection of Plates VI. and VII. than by any explanation that words can convey.

In Plate VII. the joints between the vertebræ and ribs are represented of the natural size, from the skeleton of a large boa sent from the East Indies by the late Sir William Jones, and deposited in the Hunterian Museum. On the under surface of the vertebra is a protuberance for the attachment of muscles peculiar to this genus; it varies in size in the different species, and explains the power attributed to the boa constrictor.

When the snake is going to put itself in motion, the ribs of the opposite sides are drawn apart from each other, and the small cartilages at the ends of them are bent upon the upper surfaces of the abdominal scuta, upon which the ends of the ribs rest; and as the ribs move in pairs, the scutum under each pair is carried along with it. This scutum, by its posterior edge, lays hold of the ground, and becomes a fixed point from whence to set out anew. This motion is beautifully seen when a snake is climbing over an angle to get upon a flat surface.

When the animal is moving, it alters its shape from a circular or oval form to something approaching to a triangle, of which the surface on the ground forms the base.

The coluber and boa having large abdominal scuta, which may be considered as hoofs or shoes, are the best fitted for this kind of progressive motion; there is, however, a similar structure of ribs and muscles in the anguis and amphisbæna.

In the anguis the ribs are proportionally weaker; and as these have nothing to correspond with the scuta, it is probable this mode of progressive motion is less necessary to them.

The rings of the amphisbæna are a near approach to the large scuta.

Description of the Plates.

Plate VI.

A lateral view of the muscles of the boa constrictor.

AA. The straight muscles of the back.

BB. The first set of muscles which arises from the transverse process of each vertebra, and is inserted into the rib behind it close to its head.

CC. The second set.

DD. The third set.

EE. The fourth set.

FF. The fifth set.

GG. Short muscles which pass from cartilage to cartilage.

HH. A set of oblique muscles which passes from the anterior side of the bony extremity of each rib to the posterior edge of each scutum.

II. Muscles which pass from the ribs near their heads obliquely backwards, to be inserted into the skin at the edge of each scutum.

K. Muscles of the scuta.

Plate VII. Fig. 1.

An internal view of the abdominal muscles of the boa constrictor.

AA. The muscles which pass from cartilage to cartilage of the different ribs.

BB. A set of muscles which passes from the point of each rib over two ribs to the middle of the third.

CC. A similar set of muscles continued from the opposite side of the rib, passing over three ribs to the body of the vertebra.

DD. The abdominal muscles which arise from the anterior edge of each rib, and pass to the linea alba.

EE. The linea alba.

FF. The termination of the set of oblique muscles which passes from the bony extremities of the ribs to the edges of the scuta.

GG. The

GG. The muscles of the scuta consisting of two sets which decussate each other.

PLATE VII. Fig. 2.

Represents two vertebræ and portions of ribs of the large boa, to show their articulating surfaces.

a a. The process peculiar to the vertebra of the boa.

XLV. *An Attempt to analyse Cast Iron. By Professor* BERZELIUS *.

IT has for a long time been supposed that the principal difference between cast iron and steel consisted in a small quantity of oxygen being present in the former; so that cast iron would be a kind of carburetted oxide of iron, whilst steel, on the other hand, would be a carburetted metallic iron. This pretty generally adopted conjecture respecting the presence of oxygen in cast iron, probably derived from a comparison of the difference in malleability of the cast iron and the steel, will on a closer examination be found to contradict our experience respecting the affinity of iron and carbon for oxygen; for at the temperature at which cast iron is formed the carbon and oxygen ought to be changed into gaseous carbonic acid. That cast iron should be an oxidule, is also contrary to analogy drawn from other metallic protoxides, among which, as far as I know, the lowest degree of oxidation is always that which best will dissolve in acids, and which with the greatest affinity will maintain this union; but the cast iron exhibits all the properties of a fully reduced metal. This question respecting the composition of cast iron is of importance to theory; and its elucidation may prove useful when applied to the manufacturing of cast iron; but what particularly deserves attention is the different states in which cast iron can occur, together with the influence that these different states may have on the quality of the wrought iron. The greater part of those differences in cast iron originate indisputably from the different quantities of carbon dissolved by the iron in the smelting, by which an infinite variety can be produced from the greatest to the least carburetted. A quicker or slower cooling contributes also considerably to alter the colour and hardness of the cast iron, though otherwise of identical composition. But these

* From *Afhandlingar i Fysik, Kemi och Mineralogi, utgifne af W. Hisinger och F. Berzelius.* 3 H. Stockholm. 1810.

these variations seldom essentially affect the good properties of manufactured bar iron. The cast iron often acquires other differences, derived from foreign combustible substances in the ore, or in the fluxes, which may greatly alter the properties of the bar iron made from it. To discover these, and means for their separation, is a nice and interesting object for the iron works. The quantity of these combustible matters which are metallized along with the iron in the heat of the furnace, and deteriorate its quality, is in most instances so very trifling, that in analysis it easily escapes notice; and being diffused in a mass of iron proportionally infinitely greater, it maintains its hold through all the processes to which the iron is afterwards subjected to render it malleable.

The surest way for a chemist to discover these substances is, first, to examine the ore and the fluxes employed in making the cast iron, and then in the iron itself to endeavour to detect such substances as were known to be present in the ore, and likely to affect the quality of the bar iron made therefrom.

This mode it is probable would also be the safest for the miner to pursue, in searching for means to separate on the large scale unfriendly substances from the ore, when he wishes to gain good iron from inferior ore.

But before we proceed to investigate the composition of cast iron of a bad quality, it is plain that we ought to know the constituent parts of cast iron in general, and the causes which operate to produce iron of different qualities from the same ore; namely, the different phænomena in the furnace, the duration of the smelting, the different qualities of the coal or ore made use of, &c.—I will begin with reciting an analysis of a specimen of good cast iron, containing manganese furnished by Mr. Svedenstjerna from a blast-furnace in Lekebergslag.

One of the greatest obstacles we meet with in the analysis of cast iron, is to ascertain the quantity of carbon which it contains. The difficulty of obtaining an accurate result, is no doubt the reason why our knowledge of the true composition of cast iron has hitherto remained so defective. When we dissolve the cast iron in an acid, the carbon in the moment of parting with the iron enters into new combinations, partly with the component parts of the cast iron, partly with hydrogen; nay, even with the component parts of the acid; and therefore the products vary according to the acid employed.

a. On the dissolution of cast iron by sulphuric acid, there

there is disengaged, as every one knows, an ill-scented inflammable gas, consisting of carburetted hydrogen gas containing a portion of a fetid volatile oil. This oil, formed from the carbon of the iron with oxygen and hydrogen from the water, deposits in the conducting glass-tubes in the form of a fat grayish coating, which at last swims on the water in the pneumatic trough as a thin iridescent pellicle. The solution itself posseses the smell of this oil; and the black substance remaining after the dissolution of the iron contains a great quantity of oil, which can be extracted by alcohol. Vauquelin has found that a phosphuretted iron produces a still more fetid oil, and that phosphorus also remains in this oleaginous black residuum. If this black fetid substance be burnt, there finally remains a grayish siliceous earth.

b. When cast iron is dissolved in muriatic acid, then more gas and less oil are obtained; the portion that remains insoluble in the acid is now brownish, less in weight and volume, and assumes a light gray colour on drying. It blackens when burned, and leaves at last siliceous earth. (I know not whether the difference in the quantity of the products resulting from the dissolution of the carbon in these acids, ought to be ascribed to the greater tendency of sulphuric acid to form oil and ether, than muriatic acid possesses, judging from their effect upon alcohol.) We thus find that neither the sulphuric nor the muriatic acids can be employed for ascertaining the carbon contained in iron; and it will likewise be seen from what follows, that no direct determination can be obtained from a dissolution in any of the other acids.

c. Having perceived in the experiments with the sulphuric and muriatic acids, that the affinity of carbon for the disengaged hydrogen gave origin to the different substances which were formed, I dissolved cast iron in nitro-muriatic acid. When I conducted the disengaged gas through lime-water, there occurred a considerable deposit of carbonate of lime—a proof that the carbon was acidified by the acid. During the dissolution small bright metallic particles separated from the pieces of cast iron, which pieces afterwards required a more concentrated acid and a higher temperature for dissolution. These particles were found to be graphit, which must have been deposited by the consolidation of the cast iron, and therefore now only mechanically separated, since the surrounding iron is sooner dissolved by the acid than the graphit. When all that was metallic had become dissolved; then, both from a solution with nitric acid alone, and from that with nitro-muriatic acid, there ultimately remained a black-brown, soft, carbonaceous mass, which in its appearance was perfectly alike from different experiments; but varied much in chemical habitudes, according to the quantity and concentration of the acid, the temperature at the dissolution, and other circumstances. It amounted on an average to more than 4 per cent. of the weight of the iron employed. It was always in a small degree soluble in water, and in its chemical character resembled the extractive substance met with in mould earth—the last stage of vegetable putrefaction. In some experiments, I found it so soluble in boiling water, that on cooling a great quantity was again precipitated; caustic ammonia dissolved it more copiously than water. In other experiments, it was less soluble in water, but then proportionably more so in caustic alkali, with which it formed a dark, opake, and nearly black fluid, which, like the alkaline solution of the extractive substance before mentioned, was again precipitated by acids. If an aqueous solution of this brown substance be evaporated, it then precipitates, and the liquor gradually loses its colour. On heating this substance, it smelt like heated peat turf, and when inflamed it spontaneously continued in a state of ignition, giving a smell of peat; after which there remained grayish-red ashes. In short, the action of the nitric acid on the carbon of the cast iron generates here a substance, which in character is identical with the extractive substance from mould earth and peat turf. I do not introduce this as an essential circumstance relative to the composition of cast iron, but as a remarkable phænomenon of the action of nitric acid on carbon, which perhaps might be classed along with Hatchett's discovery of artificial tannin, from the mutual action of charcoal and nitric acid. A part of this extractive substance dissolves in the last water of edulcoration, and therefore it has passed into the solution of iron undissolved. If this be precipitated with caustic ammonia, then the remaining solution is sometimes yellow; this substance does, however, for the most part precipitate with oxide of iron. When this is digested with caustic potash the ley will be of a dark-brown colour, though none of the oxide of iron is dissolved. These circumstances are sufficient to show the impossibility of determining the quantity of carbon by the solution of iron in nitric acid.

I noticed a constant peculiarity of this extractive substance, that though digested with diluted muriatic acid, so that

that all substances soluble by an acid ought to have been taken up; yet I found, after ignition of the undissolved mass, a portion of magnesia, which frequently amounted to the whole of what was contained in the iron, and which is never left undissolved when sulphuric or muriatic acids are employed without any addition of nitric acid. The magnesia, or its base, must therefore have entered into the composition of this substance, as is the case in vegetables, from which the various earths met with in the ashes never can be separated by acids before combustion.

1. A piece of cast iron (part of the same mass that was employed for all the analyses hereafter described) weighing 19·1 grammes, was dissolved in a mixture of sulphuric acid, nitric acid, and water. The dark-brown solution produced was strained through a weighed filter, and the black-brown mass left on the filter was first washed with water and nitric acid, to remove all the iron, and then with boiling water. That which passed through during the washing with boiling water deposited a brown substance on cooling. The dried filter had gained 0·225 of a gramme of adherent brown extractive substance, of which 0·15 could be taken from the filter without injuring the paper: this when burnt left 0·08 of a gramme of gray-red ashes corresponding to 0·045 for the 0·225 of extractive substance. These ashes when boiled with muriatic acid left 0·025 of silica, equal to 0·037 for the whole mass. Out of the solution saturated with caustic ammonia was precipitated a vestige of iron by benzoate of potash, after which carbonate of potash separated, on boiling, a small quantity of magnesia, but too diminutive to be weighed. (Here it might be asked, how the nature of a substance can be ascertained when in so small a quantity as not to be weighed? I had had an opportunity of discovering this by an assay on a larger scale. From 107 grammes of the very same kind of cast iron, dissolved in a similar mixture of acids, I obtained magnesia in such quantity, that its proportion to sulphuric acid evidently distinguished it from lime and other kinds of earths. But as, in my experiments with the brown extractive mass, an indeterminable portion of its weight had been destroyed, I could not, even from this experiment on a larger scale, determine the quantity of magnesia therein, which at all events is extremely small.) That portion of the extractive substance which was found partly dissolved and partly precipitated in the water employed to wash the filter, weighed, after drying, 0·17 of a gramme, and produced 0·017 of ashes in character like the former. These ashes

(0·045 +

(0·045+0·017=0·062) from the whole of the extractive substance (0·225+0·170=0·395) consisted chiefly of silica about 0·05. The remainder is oxide of iron, magnesia, and a loss which is inevitable in such small experiments.

2. The red-brown acid solution of iron was carefully neutralized with caustic ammonia: a pale yellow precipitate was slowly deposited, which I took to be a mixture of silica and phosphate of iron. The precipitate on the filter was well washed, dried, and ignited. On burning it emitted vapours of sulphureous acid, and left at last 3·375 grammes of a beautiful red oxide of iron. It dissolved completely in muriatic acid, and therefore did not contain silica: nor did it show any signs of precipitation with a solution of muriate of lead, even after several days. The yellow mass which deposited on the neutralization of the solution was therefore nothing but a sub-sulphate of oxide of iron.

3. The water employed to wash this precipitated sulphate of iron had deposited, after 48 hours, a salt in large crystals, colourless, and resembling alum. They had a sweetish astringent taste, and therefore from their external character I supposed them to be alum. I dissolved them in a caustic ley of potash. A great deal of ammonia was disengaged, and red oxide of iron was deposited in great quantity, which did not get darker by boiling,—a proof that the salt did not contain any oxide of manganese. The ley of potash super-saturated with muriatic acid, and carbonate of ammonia added in excess, gave no sign of turbidness. This salt therefore contained no alumina, and was a triple salt of sulphuric acid, ammonia, and oxide of iron.

4. The solution of iron which had been neutralized was mixed with caustic ammonia in excess. The precipitate taken on the filter was well edulcorated and mixed with that before obtained (in No. 3), and thereafter ignited in a platina crucible. The burnt oxide was dark-red, and weighed 22·28 grammes.

5. Three grammes of this oxide dissolved in muriatic acid left on the filter a little of gelatinated silica, which after burning weighed 0·01, answering to 22·28 oxide of iron, equal 0·075.

6. The muriatic solution was neutralized as correctly as possible with ammonia, then precipitated with benzoate of potash, and filtered. The percolated solution was precipitated with caustic potash in excess. The precipitate became black in drying, and weighed after burning 0·2. When

When dissolved again in muriatic acid, and precipitated with prussiate of potash, it produced a white precipitate, which by degrees assumed a purple tint in the air, and by the blow-pipe proved to be manganese. Caustic potash separated from the remaining solution a vestige of magnesia.

7. The alkaline solution remaining after the precipitation of the oxide of manganese produced no alumina when super-saturated with muriatic acid, and thereafter mixed with carbonate of ammonia. This cast iron, therefore, had not contained any sensible quantity of alumina.

8. The liquor of the solution which had been precipitated with caustic ammonia (see No. 4.) was now evaporated to two-thirds in a well covered porcelain vessel, after which it was mixed with oxalic acid: in 24 hours it deposited a most inconsiderable quantity of a white powder, which changed to red in drying, but after burning blackened, and had the properties of manganese. The remaining fluid mixed with sub-carbonate of potash, and boiled till all ammonia was expelled, did not deposit any thing, and had therefore not retained any magnesia.

But before I proceed to sum up the results from this analysis, I must give an account of some remarkable circumstances attending the separation of the substances met with in the cast iron. The first relates to the iron and the manganese. We know the certainty derived from the old method of using the neutral succinates, but this acid is unfortunately too expensive to be applied where the greater portion is oxide of iron. The action of benzoic acid upon the oxides of iron and manganese perfectly resembles that of the succinic acid; and Mr. Hisinger has already proved that it also may be considered as possessing certain advantages over the latter. But one circumstance deserves notice, which might lead to an incorrect result; namely, that the benzoic acid during the evaporation of its salts is partially volatilized, and leaves a basic salt in the dried mass. The benzoat of ammonia is however an exception from this, for it always becomes acid. These basic salts precipitate so much of manganese as the surplus of their base can afford, and thus produce a diminished result. But if the solution is acid, no manganese will precipitate, and iron alone is deposited, along with the difficultly soluble acid. It is plain that the free acid of the mixture must be no other than benzoic acid, in order that decomposition and precipitation may take place. The latter refers to the oxide of manganese and to the magnesia, for the separation of which I do not yet know any infallible method.

Vauquelin's

Vauquelin's proposal for precipitating the oxide of manganese with hydro-sulphuret of alkali may not at all succeed, and so much the less, as Berthollet states that the oxide of manganese with the sulphuretted hydrogen produces soluble combinations. The same is the case with alkaline sulphurets. What first is added, precipitates nothing; afterwards, with greater quantities of alkaline sulphates, there will precipitate a mixture of sulphur, oxide of manganese, and magnesia. I had long before experienced that sal-ammoniac when boiled with magnesia is decomposed to a triple salt; therefore, in the hope of turning to account this disposition of the magnesia for separating it from the oxide of manganese, I boiled a mixture of magnesia and oxide of manganese (precipitated together by caustic potash and edulcorated) with sal-ammoniac in greater quantity than would have been required for forming a triple salt with the whole mass. When on a continued digestion no more ammonia was disengaged, the mixture was filtered, and precipitated with caustic potash. The precipitate contained so much oxide of manganese that it blackened in drying. I therefore varied the experiment, so that a mixed solution of muriate of manganese, muriate of magnesia and sal-ammoniac were treated with caustic ammonia in great excess, and placed under a bell with oxygen gas; the mixture was taken out after 72 hours. It had deposited a great deal of black oxide of manganese. After filtration it was precipitated with caustic potash; the precipitate was found black after drying, and had therefore contained oxide of manganese. Thereafter I endeavoured to separate them, partly with neutral tartrat of ammonia, and partly with super-tartrat of ammonia; but neither of them precipitated thereby. Mallate of ammonia did not precipitate the magnesia, but yielded a small precipitate with manganese salts. It did not however precipitate fully, and therefore could not be employed. A solution of borax does not precipitate magnesia, but it precipitates solutions of manganese. If we mix a solution of magnesia and manganese with borax, there will be no precipitation; and should we have precipitated the manganese by itself with borax, and then add a solution of sulphat of magnesia, the precipitate redissolves. In short, the only method of tolerable satisfaction that I have found for separating them was this:—after having sufficiently burnt the mixture of magnesia and manganese, for the purpose of expelling the carbonic acid out of the magnesia, they were weighed, dissolved in diluted sulphuric acid, which left a great portion of of oxide of manganese undissolved, and then the dissolved oxide of manganese precipitated with prussic alkali; after which the magnesia was separated with caustic potash*.

This analysis gives the following products from the cast iron:

Red oxide of iron	25·660
Oxide of manganese contaminated with magnesia	1·485
Silica	1·125
Burnt extractive substance	0·336
	27·606

If 100 parts of red oxide of iron consist of 30·66 parts of oxygen, and 69·34 parts of iron†, then these 25·66 of oxide of iron correspond to 17·85 of pure iron in the 19·1 grammes of the analysed cast iron.

If 100 parts of burnt oxide of manganese consist of $29\frac{3}{4}$ oxygen and $70\frac{1}{4}$ metallic manganese, then 1·485 gramme of oxide indicates 1·043 metal of manganese. By adding the manganese to the quantum of iron, then, what is deficient in 19·1=0·11 of a gramme will be loss, and carbon. Arranged according to per centage, the component parts consist of

Iron contaminated with base of silex	93·875
Manganese	5·460
Carbon and loss	0·665
	1000· 00

The loss exhibited by the foregoing analysis is too small. I therefore thought it probable that a great portion of sulphuric acid, notwithstanding the strong ignition, might have remained in the oxide of the iron; which determined me to repeat the analysis in a different manner.

1. To ascertain in a positive manner the quantity of carbon present in the cast iron, I pulverized a part of the bruised crystals in a cast-iron mortar, and sifted the powder through a fine linen cloth. I did not collect what first went through, and which might possibly be defiled by dust from the

* Since these experiments were made in the beginning of the year 1808, M. Gay-Lussac has proved that the most part of the metals are precipitated from their neutral solutions in acetic acid, by means of a stream of sulphuretted hydrogen, which also may be happily employed for separating oxide of manganese from magnesia and from lime. Berthollet's abovementioned soluble compound of sulphuretted hydrogen and oxide of manganese is not formed when hydrosulphuret of potash is mixed with a solution of manganese, and Vauquelin's proposed method is of course very applicable to the same purpose.—The Author

† See the author's Experiments on Definite Proportions, *Annales de Chimie*, 1811, August; and also *Afhandlingar i Fysik, Kemi och Mineralogi.* 3 H. 1810. pag. 220.

the linen. Five grammes of this powder I mixed with 60 grammes of pure and recently crystallized nitre, which held no mechanical intermixture of combustible substances. The mixture was put in a retort of glass luted to a tubulated receiver, from whence the disengaged gas was conducted through lime-water in several bottles. The retort was heated in a sand cupel. A little before the commencement of the ignition, gas was disengaged, which made the lime-water turbid; an effect which lasted as long as any disengagement of gas took place, and which continued slowly during the ignition for four hours; after which it ceased, and the apparatus was left to cool. Carbonate of lime had deposited only in the first and second bottle. The lime-water, which in both was still caustic, was decanted, and the lime was repeatedly washed with water. It was then separated from the bottles with muriatic acid, from which it was precipitated with carbonate of ammonia. The mass which remained in the retort was alkaline, and contained at the bottom a black, somewhat brownish oxide of iron. It was dissolved in water freed from oxide of iron by filtration, and precipitated with a much diluted solution of muriate of lime. The precipitated muriate of lime mixed with that obtained from the lime-water, weighed after washing and drying 1·6 gramme, which (when 100 parts carbonate of lime contain 43·6 of carbonic acid) correspond to 0·697 parts of carbonic acid. These (since carbonic acid contains 28·46 per cent. of carbon) correspond to 0·1983 of carbon; which divided among five grammes of cast iron, produces 0·0396 of carbon per gramme, or 3·96 per cent. The dark oxide of iron obtained was difficultly soluble in muriatic acid, and gave no signs of the disengagement of hydrogen gas.

2. 7·9 grammes of the same cast iron were dissolved in nitric acid in a weighed glass phial; the solution was of a thick consistency, black-brown and opake. It was evaporated to dryness, and burnt in the phial. The cast iron had gained 2·65 grammes in weight, and left 10·55 grammes of impure oxide of iron. On taking out the oxide, there was yet found a regulus piece of iron which weighed 0 7575, and after the solution in nitric acid, and after the ignition of the nitrate of iron, weighed 1·057 gramme; so that the total oxide of iron obtained was 10·837 grammes, of which all could not be considered red oxide, since the stratum nearest the regulus must have been black oxide, and the exterior crust of the regulus must also have contained some. If we now assume that all were oxide of iron in this assay,

or,

or, what amounts to the same, that the oxide of manganese and the silica had not contained oxygen in such a proportion to the oxide of iron as to have much influenced the result, then 10·837 grammes oxide correspond to 7·542 grammes of metallic mass; after which there remain 0·357 of a gramme of loss, which in this experiment can be nothing else than carbon, and being divided among 7·9 grammes of cast iron, gives 4·5 per cent. of carbon. This must evidently be too much, when we see that the oxide obtained could not possibly be oxidated to maximum in the whole of its mass.

3. Since the result of the preceding experiment was at all events very indecisive, I repeated it with five grammes of coarsely bruised cast iron, which was dissolved completely in so much nitric acid, that the solution, when viewed against the flame of a candle, was found clear at bottom, without any iron being left undissolved. Evaporated in the same vessel to dryness, and burnt, there were left 6·925 grammes of oxide of iron. But before I undertake to determine the quantity of carbon in this experiment, I must first mention an attempt made to discover more exactly the quantity of manganese.

4. Five grammes of the same cast iron were dissolved in an apparatus for disengaging gas, with the assistance of very gentle heat. The gas was received over rain-water, and amounted to 65 Swedish decimal cubic inches; or, minus 1 cubic inch as much as from 4 grammes of malleable iron dissolved in the same apparatus, and consequently diluted with an equal volume of atmospheric air from the vessel. The gas was burnt in oxygen gas, and yielded 1·03 gramme of carbonate of lime, the carbonic acid being detained by lime-water. But in this experiment the whole apparatus was become coated with a fetid oil, which also collected on the water in the apparatus in the form of iridescent pellicles, and which admitted being washed off with alcohol, after the solution was poured out from the phial in which the solution was performed. We thus find it impossible that the whole quantity of carbon in the iron could be contained in the gas; besides that, on the combustion of the gas, the last portions were certainly prevented from complete deflagration by the copiously formed carbonic acid, which also the altered colour of the flame appeared to indicate. The 1·03 of carbonate of lime obtained, answers precisely to 2¼ per cent. of carbon. The muriatic solution was filtered, and left on the filter a gray, bulky, pulverulent mass, which after

washing

washing and burning weighed 0·0125, and was silica, which as near as can be corresponds to the silica left undissolved in the before described experiment on a larger scale. The filtered solution was mixed with nitric acid, and boiled to the full oxidation of the iron; after which it was saturated as perfectly as possible with carbonate of potash, and then precipitated with benzoate of potash: the precipitate was separated on the filter, and the filtered solution, mixed with the inspissated water of edulcoration, was precipitated with caustic potash. The deposit soon blackened. It weighed after washing and burning 0·335 of a gramme. Dissolved in muriatic acid, and precipitated with prussiate of potash, there subsided prussiate of manganese, which was collected on the filter. From the percolated fluid, caustic potash precipitated a small quantity of magnesia, which when burnt weighed 0·01 of a gramme.

In the 6·925 grammes of oxide remaining after the oxidation of five grammes of cast iron (No. 3.) there was therefore 0·325 of oxide of manganese, 0·01 of a gramme of magnesia, and (according to the before described larger scale of assay, from which I could be tolerably sure of its charge of silex) 0·0325 of silica. As to the quantity of oxygen in both these earths, it could very well be totally excluded, since the whole quantity of both of them together does not fully amount to 1 per cent. But I know from experiments of my own (though not yet in a state for publication) that the silica contains more oxygen than the red oxide of iron; and that the magnesia contains less. In the calculation I will let the one make up for the other, since the error will be but an insignificant fraction.

The case of the oxide of manganese is not so indifferent. Fourcroy states that 100 parts of manganese take up 68 parts of oxygen for forming 168 parts of black oxide of manganese. I do not know whether he in this place means the crystallized black oxide produced by nature, and which on burning gives oxygen gas; but which also I do not know that any chemist hitherto has produced artificially without the aid of electricity. Bergman estimates the quantity of oxygen in the brown oxide at 26, and the metal at 74 per cent. I reduced a small portion of carbonate of manganese free from iron, and obtained a regulus which after a careful cleansing of its surface weighed 0·5075 of a gramme. It was not in the least attracted by the magnet. Dissolved in pure nitric acid in a weighed glass phial, it yielded, after the evaporation of the salt and its calcination in the phial, 0·7225 of a gramme of black oxide of manganese.

manganese. This makes an addition in the weight of 0·215 oxygen. Thus 100 parts of oxide of manganese after exposure to a red heat consist of 70·25 of manganese and 29·75 of oxygen, and 100 parts of metallic manganese take up 42·16 parts of oxygen in forming this oxide. This so nearly agrees with the red oxide of iron, that, without any remarkable error, we may calculate the quantity of metal by omitting the trifling portions of manganese oxide, and counting the whole for oxide of iron, as I have done in the first and less successful attempt to oxidize the cast iron with nitric acid. A want of the metal of manganese prevented me from repeating this experiment on a greater quantity, to attain greater precision. But the result as stated is sufficiently accurate for the calculation we here require. I also must observe, that the oxide of manganese obtained in the analysis of the iron was afterwards treated with nitric acid in a crucible of gold, on which its weight was found not to have altered in the least; so that the manganese in both instances was in an equal degree of oxidation; which is a circumstance of great moment in the calculation.

When from 6·925 of oxide from the cast iron we deduct 0·325 oxide of manganese, there remain 6·6 grammes for oxide of iron (contaminated with a most minute portion of earths), which correspond to 4·5764 of pure metallic iron; and as 0·325 of a gramme of oxide of manganese indicate 0·2283 of metallic manganese, they make together 4·805 grammes of metal. If these be deducted from the five grammes of cast iron employed, there remain 0·195 of a gramme for carbon, which during the experiment had escaped in the form of carbonic acid; and this divided into five gives 3·9 per cent. which is 0·06 per cent. less than in the experiment for oxidizng the cast iron with nitre. These experiments may therefore be considered as perfectly corroborating each other. They also prove that the carbon in cast iron is not present in the state of diamond, but in the state of graphit, or such carbon as is contained in carbonic acid to the amount of 28 per cent.; otherwise the loss in the last calculated experiment ought to have been much less than the quantity of carbon in that with nitre. That it turned out greater in this experiment is, because a loss could not take place in it but from erroneous data for calculating the quantity of metal.

In order to prove decidedly that this cast iron contained neither sulphur nor phosphorus, I dissolved five grammes thereof in pure nitric acid; the solution was correctly saturated with caustic ammonia, and divided into two equal parts. The one was mixed with muriate of baryt, and showed no sign of precipitation even after several days. The other was mixed with a solution of acetate of lead, without the least appearance of phosphat of lead.

That the cast iron did not contain any chrom, I found when a portion of the cast iron, oxidized with nitric acid, from experiment second, was heated in a platina crucible with caustic potash. The solution thereof in water was at first green, but soon precipitated a little manganese, and became colourless; nitrate of the black oxide of mercury, after the saturation of the alkali by nitric acid, left a scarcely discoverable colourless precipitate, which, on being heated in a small crucible of gold by a blow-pipe, volatilized without residue, and was derived from a small quantity of muriatic acid in the alkali made use of.

Thus, according to the last described assay, this cast iron consisted of

Metallic iron, contaminated with the bases of silica and magnesia	91·53
Metallic manganese	4·57
Carbon	3·90
	100·00

Cast iron produces when dissolved in acids 0·0065 of its weight, or somewhat more than $\frac{1}{2}$ per cent. of silica, and 0·002, or $\frac{1}{5}$ per cent. magnesia. I could not find therein any traces of lime, or of alumine, although their presence was on good reasons expected. I am inclined to believe that the existence of oxygen in cast iron is by this analytical investigation now satisfactorily refuted, since the quantity of carbon compensates for all the deficiency of metal, and since it is certain beyond dispute, that cast iron does not contain the earths of magnesia and silica, but the metalline bodies which constitute their bases.

XLVI. *Researches respecting the Action of Benzoates with the earthy and metallic Salts.* By W. Hisinger*.

Professor J. Berzelius having suggested in his Treatise on the Sebacic Acid†, that the benzoates, like the succinates, might with certainty be employed, under similar circumstances, for separating iron and manganese from their

* From *Afhandlingar i Fhysik, Kemi och Mineralogie utgifne af W. Hisinger och F. Berzelius.* 3 H. Stockholm. 1810.

† *Loc. cit.* vol. i. p. 171 & seq.

solutions;

solutions; and this subject being of great moment for analytical chemistry, I undertook to examine more closely the changes which benzoates occasion in solutions of the earths and metals.

I must first remark, that the solutions of all the earths and metals employed were to the utmost saturated and neutralized with pure ammonia, and diluted with water. That the benzoate had been prepared from pure, sublimated and white acid, is superfluous to mention, as also that all the earths and metals made use of possessed the highest degree of purity.

I. *Benzoate of Ammonia with earthy Salts.*

1. In a solution of nitrate of *baryta* no precipitation took place, when some drops of *benzoate* of ammonia were added.

2. In a muriatic solution of *strontia*, no precipitation.

3. In muriate of *magnesia*, no precipitation.

4. With nitrate of *yttria*, a white precipitate is produced, but which soon redissolves on diluting and shaking the fluid.

5. With muriate of *lime*, there is produced a white precipitation which is somewhat difficultly soluble, but which on diluting and shaking disappears.

6. In a solution of alum saturated with ammonia there will be found a white precipitate, which dissolves slowly when greatly diluted. In a diluted and neutralized solution of sulphate of alumina, the benzoate of alumine dissolves with facility in the cold, by agitation with beryl and zirkonia. I have not been enabled to learn its action, owing to the want of these earths.

II. *Benzoate of Ammonia with Metals.*

1. With a solution of *gold*, no precipitation.

2. With a solution of *platina*, no precipitation.

3. With nitrate of pure *nickel*, no precipitation.

4. With sulphate of *zinc*, no precipitation.

5. With nitrate of *cobalt*, no precipitation.

6. In *tunstat* of ammonia, no precipitation.

If a little nitric acid be added, there will be a copious white precipitate, which in water is nearly insoluble.

7. With *molybdat* of ammonia, no precipitation.

8. With muriate of *manganese*, no precipitation.

9. With nitrate of *silver*, a white precipitate which easily dissolved by agitation.

10. With acetate of *lead*, a white precipitate which easily redissolves.

11. With acetate of *cerium*, a white precipitate easy of solution.

12. With *palladium* dissolved in nitro-muriatic acid, a white precipitate, which on diluting the solution dissolved gradually.

13. With nitro-muriate of *antimony*, it yields a white precipitate, which by degrees dissolves, if the menstruum has been much diluted.

14. Muriate of *titanium* gives a blue-gray precipitate, which dissolves if the quantity of the precipitant be not too great.

15. With nitrate of *bismuth*, a white precipitate, which is partially dissolved when the solution of this metal is not neutralized.

16. Muriate of *tin* yields a white precipitate, which dissolves slowly, and only when a small quantity of the precipitant is added.

17. Muriate of *copper*, pure and free from iron, gives a white precipitate which dissolves tardily when a little of the precipitant is added. A greater portion thereof leaves the greater part undissolved, and pale green.

18. Muriate of *tellurium:* a white precipitate almost insoluble.

19. Nitrate of *quicksilver:* a white insoluble precipitate; crystalline, consisting of small points or needles.

20. Muriate of *iron:* an orange, insoluble precipitate.

III. *Benzoate of Iron.*

a. One gramme of pure brown-red oxide of iron, previously heated to redness, was dissolved in muriatic acid with the assistance of a gentle heat. The solution was diluted with water, and was neutralized as accurately as possible with caustic ammonia; and benzoate of ammonia was added as long as any deposit could be produced. The precipitate, which was bulky, was, while wet, of a pale reddish yellow colour. It was gathered on a clean, well dried, and nicely weighed filter. The precipitate was carefully edulcorated with water, and dried by standing for several days on a heated stove, until it had ceased to diminish in weight. The colour remained the same as in its wet state, only somewhat paler. After having assumed the temperature of the apartment (+17° of the centigrade thermometer), it weighed 4·00 grammes. Thus 100 parts of *air-dried* benzoate of iron contain 25 parts of *red oxide of iron*, and 75 parts of benzoic acid and water.

b. The

b. The fluid after the precipitation with benzoate of ammonia was colourless.

1. With a few drops of sulphuret of ammonia there occurred only a pale yellowish gray, and after 24 hours there was deposited a little dark yellow sulphur.

2. With caustic ammonia in excess, there was no turbidity or dullness.

3. With tincture of galls made with alcohol,—at first no alteration, but after several hours a weak grayish colour appeared.

4. With succinate of ammonia, no alteration.

5. With prussiate of ammonia,—did not become blue, but only somewhat muddy, and after a long time deposited a pellicle of Prussian blue.

c. One gramme of dry benzoate of iron was heated for half an hour, whereby it at first swelled, smoked, burned and charred. A dark-red oxide remained, which dissolved completely in muriatic acid; it weighed full 0·24 of a gramme, agreeing, except a centigramme, with the proportion in the former experiment (*a*). The difference was owing to a smaller charge of oxygen in the oxide heated to redness along with the acid.

d. Ten parts (about five grammes) benzoate of iron were mixed with six ounces of water, and placed in a heat of +17 to 18° for 24 hours; during which the phial was often shaken. Gathered on a clean, dried, and accurately weighed filter, it showed after drying a loss of nearly $\frac{1}{10}$th of a gramme. On the evaporation of the water, there remained faint traces of red oxide of iron that could not be weighed, besides a small portion of benzoic acid.

e. Fifty parts of benzoate of iron were boiled one hour with four ounces of water. The salt was partly unaltered, partly decomposed to oxide, and also in part dissolved. Boiling water produces therefore a more speedy and more general effect on this benzoate than cold water.

f. Benzoate of iron dissolves in moderately strong muriatic acid, when agitated. A surplus of acid should therefore be carefully avoided in all precipitations with benzoates.

g. 25 grammes of benzoate of iron were mixed and agitated in a closed phial with about one ounce of caustic ammonia. After resting 12 hours on a warm stove, the clear benzoate of ammonia was by filtering separated from the oxide of iron, which, after being collected and washed on a nicely weighed filter and well dried, weighed 6·50 grammes, which (minus 0·25 grammes) coincides with the

 pro-

proportion of dried oxide of iron in the first experiment (*a*), and also with that of calcined oxide; this product being a hydrat, which its appearance indicated. It was not red-brown but pure brown externally, looking black within, and baked together in hard pieces.

IV. *Attempt to separate Iron with Benzoate of Ammonia.*

a. 0·20 of a gramme of red oxide of iron was dissolved in muriatic acid, and mixed with a solution of pure *alumina* in sulphuric acid, where the weight of the alumina was several times greater than that of the oxide of iron. The mixed solution was neutralized with caustic ammonia, diluted with water, and benzoate of ammonia added until a portion taken therefrom did not with tincture of galls discover any iron therein. The fluid was agitated now and then, and after the space of some hours was filtered. It did not contain any sensible traces of iron. The benzoate of iron was well washed with cold water, dried, and after a strong calcination left the 0·20 of a gramme of oxide of iron employed.

b. 0·35 gram. of red oxide of iron was mixed with 0·10 of a gramme of oxide of *nickel*, previously purified, free from iron and dark gray, and the mixture was dissolved in muriatic acid. The solution was diluted with water, accurately neutralized with ammonia, and precipitated with benzoate of ammonia. The precipitate was well washed, and left after a strong calcination precisely 0·35 of a gramme of red oxide of iron. The iron was consequently to the utmost nicety precipitated, and separated from the nickel.

c. A similar experiment with pure *cobalt* and oxide of iron had the like result.

d. 0·10 of a gramme of red oxide of iron and 0·15 gram. of calcined pure oxide of *manganese* were dissolved in diluted muriatic acid. The surplus of acid was saturated with caustic ammonia. Benzoate of ammonia was added until all precipitation ceased; this was taken on the filter, which was correctly weighed, was washed well with cold water, dried, and calcined strongly. The residue was a pure dark red oxide of iron weighing 0·39 of a gramme. The deficient 0·01 of a gramme was contained in what had stuck to the filter and the crucible. The oxide did not indicate any contamination of manganese.

e. 0·80 of a gramme black oxide of *manganese* and 0·10 of a gramme of oxide of iron were dissolved in muriatic acid. The solution, diluted with water, was neutralized until a commencement of turbidity, with caustic ammonia and the oxide

oxide of iron, then precipitated with benzoate of ammonia. This precipitate, washed and calcined, left within a trifle 0·10 of a gramme red oxide of iron. The solution was evaporated to dryness. On the redissolution of the remaining salt in water, there appeared some traces of oxide of iron, derived from benzoate of iron, which during the washing could have been dissolved, and there was besides a multitude of black and light traces of coal, from the destruction of the benzoate acid during the drying of the salt. After these had been separated, the solution was precipitated with carbonate of potash by digestion, which produced white carbonate of manganese; this became black after a strong and long calcination; joined to what had stopt on the filter, it weighed 0·795 of a gramme. Dissolved in muriatic acid, it showed no remarkable traces of iron by re-agents.

It appears from these experiments, that the method proposed by Professor Berzelius, to separate iron and manganese by means of benzoates, not only answers perfectly the purpose, but also presents to us a hitherto unknown agent for the most accurate purification of nickel, cobalt, and most of the metals and earths from iron, on the sole condition that the solutions are precisely neutralized and diluted, while at the same time the iron is oxidated to maximum. The influence of this discovery, on all analytical experiments, where iron so frequently occurs and remains so stubbornly, must be evident to all who are in the practice of such labours. It should however be noticed, that after having precipitated the iron from a solution containing several kinds of earth and oxides of metals together, then the benzoate added in excess ought to be destroyed by boiling the solution with some acid, to obviate any confusion caused by the benzoic acid on the continuation of the analysis.

XLVII. *On some Combinations of Platina. By* Edmund Davy, *Esq., of the Royal Institution. Communicated by the Author.*

[Continued from p. 220.]

2. *Of the Precipitate obtained from aqueous Solutions of Platina by phosphoretted Hydrogen Gas.*

In my former experiments on the combinations of platina with phosphorus*, I did not ascertain the agency of phos-

* See Philosophical Magazine for July.

phoretted hydrogen gas on solutions of this metal. I have since made an experiment of this kind; and though it offers but little novelty of result, it may not be improper briefly to describe the effects produced.

When phosphoretted hydrogen gas is passed through an aqueous solution of muriate of platina, the gas appears to be partially decompounded at the surface, it loses the property of spontaneous inflammation. The globules are detained at the surface of the fluid, and arrange themselves somewhat in the form of a honeycomb; and a yellowish-brown substance is at length deposited, which shortly after acquires a gray colour. If the gas is rapidly passed through the solution, it inflames as in other cases; but the moment this effect is produced, a grayish metallic-like substance partially covers the surface, which appears to be a phosphoret of platina.

The mode I adopted to procure this substance was similar to that used in the case of the hydrosulphuret of platina. It is necessary to use some precautions, in operating on this gas in considerable quantities, and therefore the retort should be previously exhausted twice from hydrogen gas. A short time after the retort had been filled with phosphoretted hydrogen gas, a gradual absorption of the gas took place, and a yellowish-brown coloured substance partially lined the sides of the retort, and the surface of the fluid, which slowly increased in quantity, and after some time acquired a gray colour. Whether this yellow substance is a distinct compound, or for the most part phosphorus, I have not ascertained: the latter idea appears the more probable. Whilst the hydrogen is employed in reducing the metallic oxide, the phosphorus seems to be deposited in a minute state of division, and to combine slowly with the revived metal. After the gray-coloured substance had been well washed with distilled water, and dried in hydrogen gas at a temperature about 212°, I examined it so far as to satisfy myself respecting its constitution. It was in the form of small curved laminæ. It had a slightly acid taste, evidently owing to the presence of a little uncombined phosphorus, which had absorbed oxygen during its exposure to the atmosphere. It was for the most part insoluble in the mineral acids. When heated to redness in close vessels, it furnished a gray semifused porous mass having the metallic lustre, water, and a little hydrogen gas. The hydrogen, it would seem, must have resulted from the decomposition of a little water. The gray-coloured substance in its general

general characters resembled precisely the sub-phosphoret of platina formerly described.

Being desirous of ascertaining the effect of carburetted hydrogen gas on solutions of platina, a retort containing an aqueous solution of platina was filled with carburetted hydrogen gas obtained from acetate of potash. After some time, the fluid acquired a purple tint, a slight film of a blueish-gray colour covered the surface, and there was an absorption of the gas. In an experiment of this kind, the results were examined. The substance deposited, appeared to be merely a mixture of carbonaceous matter and metallic platina.

The precipitates obtained by the agency of phosphoretted and carburetted hydrogen gases on solutions of platina were not deemed worthy of a more minute examination. I must not forget to notice, that in these experiments I was kindly assisted by my friend Mr. William Moore.

3. *Of the Combinations of Platina with Oxygen.*

The methods which are employed with success to procure the oxides of the common metals, wholly fail in the case of platina. The difficulty of obtaining pure oxides of this metal arises from the property it has of forming triple compounds with alkalies or earths, and chlorine or muriatic acid. A want of due attention to this circumstance has occasioned several erroneous statements in the writings of some distinguished chemists. Thus Bergman, in his early Treatise on Platina, states that a calx of this metal may be obtained by the agency of mineral alkali on a solution of platina*. He says also, that it may be procured by the use of the volatile alkali in particular circumstances. The various trials that I have made with these bodies in different states, lead me to conclude that they are not oxides, but peculiar triple compounds.

M. Proust † says, that a pure oxide of platina may be obtained by treating the potash muriate of platina with potash, and neutralizing with sulphuric acid; but I have not seen the details of the experiment. When dry potash muriate of platina is boiled with a strong solution of caustic potash, it is dissolved; and when neutralized by sulphuric acid, it affords a substance of a dull yellow colour, which, when washed and dried, yields chlorine gas by being heated to redness, and is partially decomposed: another compound, of a dull yellowish-brown colour, may also be procured by

* Physical and Chemical Essays, vol. ii. p. 174.

† *Annales de Chimie*, tome xlix. p. 179.

boiling

boiling the materials to dryness, and treating with water; it is a soluble triple compound, which crystallizes, and only differs, I presume, from the former in the proportions of its constituent parts.

Mr. Chenevix states the existence of two oxides of platina*, one of a yellow colour containing 13 per cent., the other of a green colour containing 7 per cent. of oxygen. The yellow oxide is said to have been procured by treating a solution of nitro-muriate of platina with lime-water, redissolving the precipitate in nitric acid, evaporating to dryness, and decomposing the sub-nitrate by heat. The green oxide, by exposing the yellow oxide to a high temperature, so as to expel a part of its oxygen.

These experiments I carefully repeated; but the results I obtained, do not at all correspond with the statements of Mr. Chenevix. For this reason it will be proper to give some of them in detail.

Experiment 1.—80 grains of pure platina were dissolved in boiling test nitro-muriatic acid, composed of three parts of muriatic and one part nitric acid: the excess of acid was expelled by heat, and the muriate began to crystallize. It was now dissolved in distilled water. To a quantity of this solution, lime-water was added until the fluid rendered turmeric paper brown; but no precipitate took place. It was digested for some time at a moderate heat on a sand-bath, but there was no change produced.

Experiment 2.—A quantity of pure platina was dissolved in nitro-muriatic acid, and the metal was occasionally added until the acid was completely saturated, and metallic platina remained at the bottom of the vessel: to the clear filtered solution, lime-water was added until the colour of turmeric paper was changed by the fluid, but no precipitate whatever appeared. The only instance in which I obtained any precipitate by lime-water, was in the following experiment.

Experiment 3.—A saturated solution of nitro-muriate of platina was boiled to dryness, redissolved in water, and the aqueous solution evaporated until a yellow precipitate began to be deposited. It was then passed through a filter, and was gradually treated with 41 times its volume of lime-water; after which, it rendered turmeric paper brown. After some minutes a very minute quantity of a yellowish flocculent substance remained suspended in the fluid, which in about three hours precipitated. Several hours afterwards the solid precipitate was collected on a filter, and after being

* Phil. Trans. vol. xciii. p. 314.

washed

washed with distilled water and dried, it weighed $2\frac{5}{10}$ths grains. It was of a brown colour. It was exposed to a red heat in a small retort over mercury, but no gas came over. It was now of a blackish colour, and in the form of small grains. It had a strong saline taste resembling muriate of lime, and deliquesced by exposure to the air for a very short time. It seemed to be a compound of muriate of lime and muriate of platina, but I did not examine it minutely.

I varied these experiments, and occasionally repeated them at different times with similar results. I also made many other experiments to procure the pure oxides of platina; but as they were unsuccessful, I shall merely relate such of them as furnish some novelty of result.

I heated potassium with platina, in a finely divided state, in a small platina tray; the potassium entered into vivid ignition, and a yellow insoluble substance was formed, which furnished oxygen gas when heated: but though it had been well washed with distilled water, it still exhibited distinct traces of the presence of alkali after being heated. The quantity I procured of this substance was too minute to determine its component parts; it seems to be a compound of oxide of platina with potash. It appears very likely that the pure oxide of platina might be procured from this substance by particular treatment.

The potash muriate of platina, when heated with finely powdered caustic lime, furnishes oxygen gas, muriate of lime, and a black powder which yields oxygen gas by heat, and appears to be a black oxide with probably a little metallic platina.

The insoluble compound of chlorine and platina, when boiled for some time with a strong solution of caustic soda, affords a black powder, which, when well washed and dried, furnishes oxygen gas and chlorine by heat, and seems to be a mixture of black oxide of platina with that insoluble compound.

At the suggestion of Professor Berzelius, I digested some of the insoluble compound of chlorine and platina, in a strong solution of caustic potash, for a considerable time, at a moderate heat. A black powder was obtained analogous to that from soda, but it was not a pure oxide; it furnished both oxygen and chlorine by heat. This distinguished chemist has recently made some interesting experiments on the oxides of platina, which I presume will soon be published: he has determined from calculations the proportions of oxygen they contain; but I believe he has not obtained these oxides in a state of purity.

Whilst I was engaged in these inquiries, my friend Mr.

Mr. J. Sowerby junior put into my hands a compound he had obtained, in attempting to destroy some diamond powder by the agency of nitre in a platina crucible. A quantity of nitre was mixed with diamond powder, and exposed to a low red heat for some time in a common fire; on examining the results, it was found that the crucible had lost several grains in weight. After the separation of the residual diamond powder, the substance in question remained. It was washed with distilled water, and on examination exhibited the following properties. Its colour was black. It was in small grains. It had a shining lustre resembling blende. It was tasteless, and insoluble in water. When a little of it was heated to redness on a slip of platina, oxygen gas was disengaged, and the platina reduced to the metallic state. It did not appear to be affected by strong nitric or sulphuric acid at a boiling heat. There was scarcely any action produced on it by boiling nitro-muriatic acid.

It was dissolved with some little difficulty in boiling muriatic acid; and when the excess of acid was expelled, the insoluble compound of platina and chlorine remained, and muriate of potash. Three grains of the black powder were heated to redness in a small glass retort; more than half a cubical inch of oxygen gas was obtained, some water, and 2·4 grains of a gray substance having the metallic lustre. This substance readily rendered turmeric paper brown. After the alkali had been separated from it by a considerable portion of distilled water, the platina weighed 1·3 grain.

From these experiments it seems that this substance is compounded of oxide of platina, potash, and water. I shall not attempt to give the proportions of its constituent parts, the quantity in my possession was too minute to determine this point with precision. This substance can only be formed at comparatively low temperatures. A more intimate acquaintance with it may probably lead to the knowledge of some interesting combinations of platina.

From the preceding statements it would seem, that at present we have no distinct knowledge of the oxides of platina in a pure or separate state; and that those substances which have been considered as oxides, are in fact compounds of this metal with metallic bases, and oxygen or chlorine.

As hydrogen is known to reduce certain metallic oxides in particular circumstances, it occurred to me, that it might probably be employed with advantage to determine the proportion

portion of oxygen in the oxide of platina. With this view I made the following experiments.

Experiment 1.—Into a retort of the capacity of about 40 cubical inches, one cubical inch of an aqueous solution of muriate of platina was introduced; the retort was exhausted of air, and filled with hydrogen gas. It was immediately removed to a vessel of water, and the stop-cock taken off; the absorption of the hydrogen, and the consequent reduction of the metallic oxide were very gradual, and it was not till after three days that the platina was all obtained in the metallic state: when carefully collected, and previously heated to redness, it weighed 7·8 grains. The hydrogen actually absorbed, amounted to 8·8 cubical inches; the barometer being at 30°, and the thermometer 60°. Now, as two volumes of hydrogen combine with one of oxygen to form water; the one half of the volume of hydrogen absorbed, will indicate the quantity of oxygen previously combined with the metal obtained. In this experiment the oxygen in union with 7·8 grains of platina, absorbed 8·8 cubical inches of hydrogen. Hence that in 100 grains would require 112·82 cubical inches of hydrogen.

For 7·8 : 8·8 :: 100 : 112·82.

And 112·82 cub. inch. of hydrogen = 56·41 cub. inch. of oxygen. According to Sir H. Davy 100 cub. inch. of oxygen gas weigh about 34 grains. Hence, 100 grains of platina will combine with 19·17 grains of oxygen.

For 100 : 34 :: 56·41 : 19·17.

And 100 grains of oxide of platina existing in the aqueous solution of muriate of platina, will consist of

84	platina.
16	oxygen.
100	

Experiment 2.—This experiment was conducted in a manner precisely similar to the above. The only difference was, that the solution of platina in this case, contained a much larger quantity of metal. 21·5 grains of metallic platina were obtained, and 25·13 cubical inches of hydrogen were absorbed. Barometer 30°; thermometer 60°. By calculation it will be found that, in this experiment, 100 grains of oxide of platina appear to consist of

Platina	83·5
Oxygen	16·5
	100·0

In

In a third experiment 8·9 cubical inches of hydrogen were absorbed, and 7·58 grains of platina obtained: the barometer being at 30°, and thermometer 60° Fahrenheit. This result agrees very nearly with the preceding one. The difference between these experiments is not very great. No allowance has been made, in the calculations, for a small quantity of hydrogen gas which would have been absorbed by the water. There is some difficulty in making correct experiments in this way, as there is often a slight film of the revived metal deposited on the sides of the retort, which adheres to it with great obstinacy, and must of course materially affect the results. It seems probable that the oxygen is rather overrated in the two last experiments; but I am inclined to think, from a comparison of all the experiments, and making every allowance, that it can scarcely be less than 16 per cent. From calculations derived from the experiments of Professor Berzelius, it appears that the peroxide of platina (which may be presumed to exist in the above solutions) contains only 14·1 grains per cent. of oxygen.

During the first of the preceding experiments, I witnessed some phænomena which seem to illustrate in a beautiful manner the attraction of cohesion. After the retort had been filled with hydrogen gas, and suffered to remain for some hours in water, a number of minute metallic particles scarcely visible to the naked eye made their appearance. By degrees they increased in size, and were perceived to be in motion. They appeared to be flowing from all parts of the circumference to the centre, where they ultimately found their place of rest, and attached themselves to a small mass of platina. As they approached the centre, they moved with greater velocity. This phænomenon continued for some time, until a considerable part of the surface was covered with the metal. It appears to succeed best when the aqueous solution of platina contains but a small quantity of metal. The most perfect instance I have witnessed was when a cubical inch of the solution contained only 7·8 grains of platina. In other cases, it may be presumed that the force of cohesion will be counteracted by the gravity which the reduced particles of metal will soon acquire.

I shall not attempt to set limits to the application of hydrogen gas, as a means of determining the quantity of oxygen in metallic oxides; or to say, whether it may not in certain circumstances revive all this class of bodies. This however seems probable, that it will offer a simple and

and accurate mode of determining the composition of certain metallic oxides, and of thus reconciling the differences which exist in the estimates of chemists.

4. *Of the Combinations of Platina with Chlorine.*

My cousin, Sir H. Davy, appears to have proved, that in the present state of our knowledge, chlorine or oxymuriatic gas is a simple body, and like oxygen it forms peculiar compounds with metallic substances*. The combinations of platina with chlorine have not hitherto been examined; they are not known in a separate state. Platina appears to form two distinct compounds with chlorine, one soluble, the other insoluble in water. I have not examined the former, which appears to be produced in an impure state, when a solution of muriate of platina is evaporated to dryness, and to be precipitated in union with horn silver by nitrate of silver. This substance is very soluble in water, and deliquescent. My experiments have been wholly confined to the insoluble compound, which, as it is unaffected by chlorine, may be presumed to contain the larger proportion of this principle. In making the details connected with the examination of this substance, I shall beg leave to adopt the nomenclature suggested by Sir Humphry Davy. Supposing the soluble compound to contain one proportion of chlorine, and the insoluble compound two proportions; the former will be called *Platinane*, and the latter *Platinana*.

Of Platinana.

This substance has been imperfectly known for some time, as the insoluble muriate of platina. In the state in which it has been examined, it appears to have been a mixture of platinana with platinane, and has been supposed to be a compound of muriatic acid, oxide of platina, and water.

When platina in a finely divided state is introduced into a retort filled with chlorine gas, no apparent action is produced; on the application of heat, they slowly combine, and a substance is formed, which passes from intermediate shades of brown to an olive colour, and there is a considerable absorption of the gas. This substance appears to be precisely similar in external characters to that obtained by a mode which will presently be described, but it differs from it materially as to the proportion of chlorine. The combination in this instance is very partial, and merely superficial. In the trials that I have made in this

* Phil. Trans. 1809, p. 373.

way,

way, I have not been able to combine platina with more than about 6 per cent. of chlorine, though the metal was in a very minute state of division, and successively heated in the gas. The circumstances unfavourable to perfect combination in the experiment, are the extreme density of the particles of the metal, the small surface exposed, and the facility with which the partial compound is again decomposed by the agency of heat *. Probably the true compound might be formed in this way, by employing extremely fine platina leaf.

The best mode I have found of procuring platinana, is to boil pure platina in test muriatic acid, and to add occasionally a little nitric acid; the solution, after being evaporated to dryness, is treated with a small quantity of muriatic acid, which is again expelled. The dry mass is then cautiously heated nearly to redness, and afterwards boiled with a considerable quantity of distilled water, to separate any platinane that may be present: after being washed and dried it is the substance in question, and exhibits the following properties.

Physical and Chemical Properties.

Its colour is dull olive brown or green. It is obtained in small lumps, which are easily reduced to fine powder. It has rather a harsh feel. It has no smell or taste. It is infusible. It is not apparently affected by the atmosphere; at least no difference could be perceived in its weight or other sensible properties, when 10 grains had been thus exposed for about four days. It is nearly insoluble in water. When it is kept in contact with this fluid, a minute portion appears to be dissolved or decomposed, and nitrate of silver occasions a slight precipitate: the effect is increased when it is boiled in water. When a little of it is kept for several days in water, a slight pellicle forms on the surface, having the metallic lustre: it is probably the platina revived. This effect seems to be connected with the agency of light, for I found that it did not take place under similar circumstances in darkness. The direct solar rays effected no change on platinana rubbed on paper, after an exposure of several hours. When heated on a thin slip of platina it assumes a dark-brown colour; at a red heat the chlorine is

* It is perhaps worthy of notice, that several of the combinations of platina in their relations to temperature, seem to exhibit remarkable peculiarities. Thus the compound of potash and oxide of platina, platinana, &c. are only formed at certain temperatures; at lower degrees of heat the compounds are not formed, and at higher they are decomposed.

expelled,

expelled, and the platina remains in a pure state. When it is exposed to a red heat in close vessels, chlorine gas is obtained and metallic platina. It is scarcely affected by test muriatic acid at the common temperature, but when boiled in this acid, a portion of it is dissolved; the solution is of a yellowish-brown colour, and when evaporated to dryness affords a dark-brown substance. This variation of colour is probably unconnected with any chemical change, and depends merely on its state of aggregation. It is slightly soluble in diluted muriatic acid at a boiling heat. Strong nitro-muriatic acid appears to dissolve only a small quantity of it. It seems to be insoluble in nitric, sulphuric, phosphoric, and acetic acids at all temperatures. Nor is the agency of these acids on it attended by any thing worthy of notice. When it is boiled in a strong solution of pure potash or soda, a black powder is obtained, which, as I have already stated, furnishes oxygen gas and chlorine by heat. When it is digested in pure ammonia, it is partially dissolved; the clear solution, when evaporated to dryness on glass, leaves a very delicate and pretty coating of platina, which on being heated to redness becomes permanent. When platinana is heated with sulphur or phosphorus, sulphurane and phosphorane, together with sulphuret and phosphoret of platina are formed. It is unaffected when heated in chlorine gas. It appears to be nearly insoluble in alcohol and ether. When it is heared with red precipitate, calomel, oxygen, mercury, and platina are the results.

The preceding statements seem to furnish distinct evidences of the constitution of this substance; that it is compounded of chlorine and platina. Its direct formation in chlorine gas, the results of its decomposition in close vessels, the products it affords with sulphur and phosphorus, are decisive as to this point. Platinana appears to be the only combination of chlorine and a metal yet examined, which affords its elements in their simplest known forms by the mere agency of heat out of the contact of air: the combination of gold and chlorine is analogous in this respect.

Analysis.

The composition of platinana could not be accurately determined by synthesis. Its decomposition in the atmosphere, in close vessels, and by sulphur, affords ample means of determining the proportions of its constituent parts; and from a comparison of experiments made in each of these ways, I shall venture to state its composition. In my first experiments on a compound of this kind, which were made at the request of Sir H. Davy, and which he has noticed in his "Elements of Chemical Philosophy," p. 451, I obtained only 18·5 per cent. of chlorine. I have since found that the substance then examined, was not platinana in a pure state, but a mixed compound, which of course could not furnish correct results.

Experiment 1.—15 grains of platinana were heated to redness in a small luted glass retort: when no more gas could be expelled, the retort was suffered to cool, and the results examined. The gas obtained amounted to three cubical inches, and was chlorine: the remainder was absorbed by the water during the process: it had a yellowish-green colour, was absorbed on being agitated in water, and had the precise odour of chlorine. The platina was in its metallic state, and weighed 10·7 grains: there was also a very minute quantity of a brown substance in the neck of the retort, which appeared to be a little of the compound expelled during the disengagement of the gas.

From this experiment, 100 grains of platinana appear to be composed of about

71·33	platina
28·67	chlorine
100·0	

For 15 : 10·7 : : 100 : 71·33.

But in this calculation no allowance is made for the small quantity of the compound found in the neck of the retort. Hence, there is reason to conclude that the quantity of platina is here under-rated, which evidently appears to be the case from the following experiments.

Experiment 2. — 10 grains of platinana mixed with flowers of sulphur and heated to redness in a small retort, afforded sulphurane, and sulphuret of platina, which, when reduced at a red heat in a platina crucible, furnished 7·25 grains of platina.

From this experiment it would appear that 100 grains of platinana contain

72·5	platina
27·5	chlorine
100	

For 10 : 7·25 : : 100 : 72·5.

Experiment 3.—10 grains of platinana exposed to a red heat in a platina crucible were entirely decompounded, and 7·25 grains of platina were obtained.

This experiment precisely agrees with the preceding one.

Experiment 4.—10 grains of platinana which had been exposed to the atmosphere for about 10 days were decompounded

pounded at a red heat in a platina crucible, and afforded 7·2 grains of platina; probably, a little moisture might have occasioned the slight difference in this experiment.

All these experiments very nearly agree: as the second and third afford the same results by different methods, they appear to merit most confidence. Hence, 100 grains of platinana consist of 72·5 platina

*27·5 chlorine

100

5. *Of the simple Salts of Platina.*

Scanty as our information is concerning the salts of platina, it will nevertheless be confined within still narrower limits, when what is erroneous in the statements of chemists is corrected. An acquaintance with the oxides of platina ought surely to precede a knowledge of their combinations with acids. If what has been stated in the preceding pages is correct, we have as yet no accurate information relative to the oxides of platina in a pure or separate state.

What is supposed to be known concerning the simple salts of platina, is principally derived from the experiments of Mr. Chenevix on the subject. He states the composition of a sub-nitrate, an insoluble sulphate, and an insoluble muriate of platina. The sub-nitrate of Mr. Chenevix is said to have been obtained, by precipitating platina from its acid solution by lime, redissolving the precipitate in nitric acid, and evaporating to dryness. This experiment I carefully repeated two or three times, but without witnessing similar results. I only procured a minute quantity of a triple compound, as I have already stated. When a solution of platina in nitro-muriatic acid is evaporated to dryness, and afterwards exposed to a pretty elevated temperature, the greater part of the nitric acid is expelled: by raising the heat still higher, it is entirely driven off, and a compound of chlorine and platina remains, which cannot be decompounded by nitric acid. No nitrate of platina can be produced by the direct agency of nitric acid on this metal; the nitro-muriate or muriate of platina, cannot be decomposed by nitric acid, nor the muriatic acid separated by nitrate of silver, or by any other method that I am ac-

* I have since operated on a much larger quantity, and obtained almost precisely the same results: 71 grains of platinana, which had been drying on a sand-bath for more than a fortnight, were decomposed at a red heat in a platina crucible, and furnished 51·5 grains of platina.

quainted with: and it does not appear that any pure oxide of platina has yet been dissolved in nitric acid, so as to furnish this salt.

The insoluble sulphate of platina, according to Mr. Chenevix, was procured by expelling the muriatic acid from the dry muriate of platina, by means of sulphuric acid, and evaporating to dryness. The results I have obtained from a careful repetition of his experiments, are very different. It will be proper to relate some of them.

Experiment 1.—15 grains of a compound of chlorine and platina (obtained by evaporating an acid solution of platina to dryness, and exposing the dry mass to an elevated temperature,) were boiled with some concentrated test sulphuric acid in a platina crucible. The process was continued until all the sulphuric acid appeared to be expelled. A substance analogous to the compound of platina and chlorine remained. A fresh quantity of sulphuric acid being added, heat was applied till no more sulphuric fumes could be perceived. The residual substance was then decompounded at a red heat, and during the process the smell of chlorine was distinctly perceived.

Experiment 2.—Several grains of a similar compound to that employed in the preceding experiment, were treated precisely in the same way with sulphuric acid. Acid was added three successive times with a little water. After the expulsion of the acid by heat, the remaining substance had the precise characters of the original compound. It was now boiled to dryness with a concentrated solution of pure soda. It became black. It was digested in distilled water. The clear solution gave not the slightest precipitate with muriate of barytes, but a very copious one with nitrate of silver. Hence, it contained no sulphuric acid.

These experiments seem clearly to prove that sulphuric acid does not decompose the compound of platina and chlorine, or the substance which has been considered as an insoluble muriate. The following experiment appears to prove, that strong sulphuric acid does not expel the muriatic acid from a solution of platina, so as to destroy the combination of chlorine and this metal.

Experiment 3.—Some platina was dissolved in concentrated nitro-muriatic acid; the solution was treated with strong sulphuric acid; a yellow substance precipitated *, and muriatic acid was expelled. After the whole had been boiled for some time, fresh sulphuric acid was added, and

* I am inclined to believe that this yellow substance is platinane. I could not obtain it in this instance so as to examine it.

the

the boiling continued to dryness. The mass was now heated just below redness in a platina crucible. It acquired an olive colour, and resembled platinana. It was heated with flowers of sulphur in a small retort over distilled water, and afforded sulphuret of platina and sulphurane, which last flowed in drops into the water: it was partially decompounded, and occasioned no precipitate with muriate of barytes, but a copious one with nitrate of silver.

From these experiments I must conclude, that no subnitrate, or insoluble sulphate of platina, can be obtained by the preceding methods; nor are any such compounds at present known to exist. The insoluble muriat of Mr. Chenevix was, I presume, a mixed compound of platinane and platinana with water. I feel a reluctance in making these and the preceding statements, because they are opposed to those of Mr. Chenevix. It would have been much more agreeable to me, to have confirmed his general results. I am convinced this ingenious chemist must have been misled by some circumstances connected with his experiments.

M. Tromsdorf* states that oxide of platina combines with benzoic acid, and forms a yellow-coloured salt. I have not seen the details of his experiments; but as the oxides of platina appear to be unknown in a separate state, and it is extremely difficult to detach chlorine or the alkalies from platina, I must regard the existence of this salt as extremely doubtful. Bergman speaks of a combination of the calx of platina with the acid of sugar†. The calx he employed was obtained from a solution of platina by soda. This substance is not an oxide of platina, as has been supposed. After being dried, it furnishes a little oxygen by heat, and a substance analogous to platinana with muriate of soda. Hence, there can be little doubt but that this oxalate of platina is in fact a triple compound of this kind, with a little of the acid.

Dr. Thomson speaks of a soluble prussiate of platina‡. I have not been able to gain any evidence of the existence of this salt. Scheele found that prussic acid had no effect on platina, or on those substances which were then considered as calxes of this metal. Bergman states that prussiate of potash occasions no precipitate in solutions of platina, and Mr. Chenevix and Dr. Thomson assert that platina is not precipitated by the prussiates. As a concentrated solution of muriate of platina appears to be a very

* *Annales de Chimie*, tome xi. p. 315.
† Physical and Chemical Essays, vol. i. p. 322.
‡ System of Chemistry, vol. iii. p. 134.

good test of the presence of potash, and has been long known to form triple compounds with some of the salts of potash and ammonia, I made experiments on all the salts of potash and ammonia I could procure, to ascertain whether there were any exceptions to such decompositions.

The aqueous solution of muriate of platina, I found, produced a yellow precipitate in the acetate, phosphate, succinate, prussiate, sulphate, nitrate, &c. of these alkalies, which appeared to be in all cases precisely similar in its sensible properties. I therefore presume, it may be stated as a general fact, that the muriate of platina occasions a precipitate in all the salts of potash and ammonia; and that the exceptions are merely apparent, arising from the use of diluted solutions, and founded on the solubility of the triple salts formed.

[To be continued.]

XLVIII. *Further Experiments and Observations on the Action of Poisons on the Animal System.* By B. C. Brodie, *Esq. F.R.S. Communicated to the Society for the Improvement of Animal Chemistry, and by them to the Royal Society*.*

I.

Since I had the honour of communicating to the Royal Society some observations on the action of certain poisons on the animal system, I have been engaged in the further prosecution of this inquiry. Besides some additional experiments on vegetable poisons, I have instituted several with a view to explain the effects of some of the more powerful poisons of the mineral kingdom. The former correspond in their results so nearly with those which are already before the public, that, in the present communication, I shall confine myself to those which appear to be of some importance, as they more particularly confirm my former conclusions respecting the recovery of animals apparently dead, where the cause of death operates exclusively on the nervous system. In my experiments on mineral poisons, I have found some circumstances wherein their effects differ from those of vegetable poisons, and of these I shall give a more particular account. Whatever may be the value of the observations themselves, the subject must be allowed to be one that is deserving of investigation, as it does not appear unreasonable to expect that such investigation may hereafter lead to some improvements in the

* From Philosophical Transactions for 1812, part i.

healing

healing art. This consideration, I should hope, will be regarded as a sufficient apology for my pursuing a mode of inquiry by means of experiments on brute animals, of which we might well question the propriety, if no other purpose were to be answered by it than the gratification of curiosity.

In my former communication on this subject, I entered into a detailed account of the majority of my experiments. This I conceived necessary, because in the outset of the inquiry I had been led to expect that even the same poison might not always operate precisely in the same manner; but I have since had abundant proof, that in essential circumstances there is but little variety in the effects produced by poisons of any description, when employed on animals of the same, or even of different species, beyond what may be referred to the difference in the quantity or mode of application of the poison, or of the age and power of the animal. This will explain the reason of my not detailing, in the present communication, so many of the individual experiments from which my conclusions are drawn, as in the former; at the same time I have not been less careful to avoid drawing general conclusions from only a limited number of facts. Should these conclusions prove fewer, and of less importance than might be expected, such defects will, I trust, be regarded with indulgence, at least by those who are aware of the difficulty of conducting a series of physiological experiments; of the time which they necessarily occupy; of the numerous sources of fallacy and failure which exist; and of the laborious attention to the minutest circumstances, which is in consequence necessary in order to avoid being led into error.

II. *Experiments with the Woorara.*

In a former experiment, I succeeded in recovering an animal, which was apparently dead from the influence of the essential oil of bitter almonds, by continuing respiration artificially until the impression of the poison upon the brain had ceased; but a similar experiment on an animal under the influence of the woorara was not attended with the same success. Some circumstances led me to believe, that the result of the experiment with the woorara might have been different, if it had been made with certain precautions; but I was unable at that time to repeat it, in consequence of my stock of the poison being exhausted. I have since, however, been able to procure a fresh supply, and I shall relate two experiments which I have made with it. In one of these, an animal apparently dead from the woorara, was made to recover, notwithstanding the functions of the brain appeared to be wholly suspended for a very long period of time: in the other, though ultimate recovery did not take place, the circulation was maintained for several hours after the brain had ceased to perform its office.

Experiment 1.—Some woorara was inserted into a wound in a young cat. She became affected by it in a few minutes, and lay in a drowsy and half sensible state, in which she continued at the end of an hour and fifteen minutes, when the application of the poison was repeated. In four minutes after the second application, respiration entirely ceased, and the animal appeared to be dead; but the heart was still felt acting about one hundred and forty times in a minute. She was placed in a temperature of 85 of Fahrenheit's thermometer, and the lungs were artificially inflated about forty times in a minute.

The heart continued acting regularly.

When the artificial respiration had been kept up for forty minutes, the pupils of the eyes were observed to contract and dilate on the increase or diminution of light; saliva had flowed from the mouth, and a small quantity of tears was collected between the eye and eye-lids; but the animal continued perfectly motionless and insensible.

At the end of an hour and forty minutes, from the same period, there were slight involuntary contractions of the muscles, and every now and then there was an effort to breathe. The involuntary motions continued, and the efforts to breathe became more frequent. At the end of another hour, the animal, for the first time, gave some signs of sensibility when roused, and made spontaneous efforts to breathe twenty-two times in a minute. The artificial respiration was discontinued. She lay, as if in a state of profound sleep, for forty minutes, when she suddenly awoke, and walked away. On the following day she appeared slightly indisposed; but she gradually recovered, and is at this time still alive and in health.

Experiment 2.—Some woorara was applied to a wound in a rabbit. The animal was apparently dead in four minutes after the application of the poison; but the heart continued acting. He was placed in a temperature of 90°, and the lungs were artificially inflated. The heart continued to act about one hundred and fifty times in a minute. For more than three hours the pulse was strong and regular; after this, it became feeble and irregular, and at the

end

end of another hour the circulation had entirely ceased. During this time there was no appearance of returning sensibility.

The circulation of the blood may be maintained in an animal from whom the brain has been removed for a considerable, but not for an unlimited time. We may conclude, that in the last of these experiments the animal did not recover, because the influence of the poison continued beyond the time during which the circulation may be maintained without the brain.

III. *On the Effects of Arsenic.*

When an animal is killed by arsenic taken internally, the stomach is found bearing marks of inflammation; and it is a very general opinion, 1. that this inflammation is the cause of death: 2. that it is the consequence of the actual contact of the arsenic with the internal coat of the stomach. But in several cases I have found the inflammation of the stomach so slight, that on a superficial examination it might have been easily overlooked; and in most of my experiments with this poison, death has taken place in too short a time for it to be considered as the result of inflammation: and hence we may conclude, that the first of these opinions is incorrect; at least as a general proposition.

Many circumstances conspire to show that the second of these opinions also is unfounded.

In whatever way the poison is administered, the inflammation is confined to the stomach and intestines; I have never seen any appearance of it in the pharynx or œsophagus.

Mr. Home informed me, that in an experiment made by Mr. Hunter and himself, in which arsenic was applied to a wound in a dog, the animal died in twenty-four hours, and the stomach was found to be considerably inflamed.

I repeated this experiment several times, taking the precaution always of applying a bandage to prevent the animal licking the wound. The result was, that the inflammation of the stomach was commonly more violent and more immediate, than when the poison was administered internally, and that it preceded any appearance of inflammation of the wound *. Some experiments are already before the public, which

* Since the greater part of my experiments on this subject were made, I have seen an account of an inaugural Dissertation on the Effects of Arsenic, by Dr. Jaëger, of Stuttgard. Dr. Jaëger has come to conclusions similar to those above stated, that in an animal killed by arsenic, the inflammation of the stomach is not the cause of death, and that the poison does not produce its fatal effects until it has entered the circulation. I have to regret that I have had no opportunity of seeing the original of this Dissertation.

which led me to conclude that vegetable poisons, when applied to wounded surfaces, affect the system by passing into the circulation through the divided veins. From this analogy, and from all the circumstances just mentioned, it may be inferred that arsenic, in whatever way it is administered, does not produce its effects even on the stomach until it is carried into the blood.

But the blood is not necessary to life, except so far as a constant supply of it is necessary for the maintenance of the functions of the vital organs. The next object of inquiry therefore is, when arsenic has entered the circulation, on what organs does it operate, so as to occasion death?

When arsenic is applied to an ulcerated surface, it produces a slough, not by acting chemically, like caustics in general, but by destroying the vitality of the part to which it is applied, independently of chemical action. This led me at first to suppose, that when arsenic has passed into the circulation, death is the consequence, not so much of the poison disturbing the functions of any particular organ, as of its destroying at once the vitality of every part of the system. The following circumstances, however, seem to show that this opinion is erroneous. In an animal under the full influence of arsenic, even to the instant of death, some of the secretions, as those of the kidneys, stomach, and intestines, continue to take place in large quantity; and the muscles are capable of being excited, after death, to distinct and powerful contractions by means of the Voltaic battery.

Experiment 3.—Seven grains of the white oxide of arsenic were applied to a wound in the back of a rabbit.

In a few minutes he was languid, and the respirations were small and frequent. The pulse was feeble, and after a little time could not be felt. The hind legs became paralysed*. He grew insensible, and lay motionless, but with occasional convulsions. At the end of fifty-three minutes from the time of the arsenic being applied, he was apparently dead; but on opening the thorax, the heart was found still acting, though very slowly and feebly. A tube was introduced into the trachea, and the lungs were artifi-

* I have observed, that where the functions of the brain are disturbed, paralysis first takes place in the muscles of the hind legs; afterwards in those of the trunk and fore legs; and last of all in the muscles of the ears and face. These facts seem to show that the influence of the brain, like that of the heart, is not propagated with the same facility to the distant as to the near organs; and this is further confirmed by cases of disease which occasionally occur, in which, although the paralysis is confined to the lower half of the body, the morbid appearances met with on dissection are entirely confined to the brain.

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cially inflated; but this appeared to have no effect in prolonging the heart's action. On dissection, the inner membrane of the stomach was found slightly inflamed.

Experiment 4.—Two drams of arsenic acid dissolved in six ounces of water were injected into the stomach of a dog, by means of a tube of elastic gum, passed down the œsophagus. In three minutes he vomited a small quantity of mucus, and this occurred again several times. The pulse became less frequent, and occasionally intermitted. At the end of thirty-five minutes the hind legs were paralysed, and he lay in a half sensible state. At the end of forty-five minutes he was less sensible; the pupils of the eyes were dilated; the pulse had fallen from 140 to 70 in a minute, and the intermissions were frequent. After this, he became quite insensible; convulsions took place, and at the end of fifty minutes, from the beginning of the experiment, he died. On opening the thorax, immediately after death, tremulous contractions of the heart were observed; but not sufficient to maintain the circulation. The stomach and intestines contained a large quantity of mucous fluid, and their internal membrane was highly inflamed.

These experiments were repeated, and the results, in all essential circumstances, were the same. The symptoms produced were, 1. paralysis of the hind legs, and afterwards of the other parts of the body; convulsions; dilatation of the pupils of the eyes; insensibility: all of which indicate disturbance of the functions of the brain: 2. a feeble, slow, intermitting pulse, indicating disturbance of the functions of the heart. Where the heart has continued to act after apparent death, I have never, in any one instance, been able to prolong its action by means of artificial respiration. 3. Pain in the region of the abdomen; preternatural secretion of mucus from the alimentary canal; sickness and vomiting in those animals which are capable of vomiting; symptoms which arise from the action of the poison on the stomach and intestines. There is no difference in the effects of arsenic, whether it is employed in the form of white oxide, or of arsenic acid, except that the latter is a more active preparation. When arsenic is applied to a wound, the symptoms take place sooner than when it is given internally; but their nature is the same.

The symptoms produced by arsenic may be referred to the influence of the poison on the nervous system, the heart*, and the alimentary canal. As of these the two former

* When I say that a poison acts on the heart, I do not mean to imply that it necessarily must act directly on the muscular fibres of that organ. It is highly

former only are concerned in those functions which are directly necessary to life, and as the alimentary canal is often affected only in a slight degree, we must consider the affection of the heart and nervous system as being the immediate cause of death.

In every experiment which I have made with arsenic, there were evident marks of the influence of the poison on all the organs which have been mentioned; but they were not in all cases affected in the same relative degree. In the dog, the affection of the heart appeared to predominate over that of the brain; and on examining the thorax, immediately after death, this organ was found to have ceased acting, and in a distended state. In the rabbit, the affection of the brain appeared to predominate over that of the heart, and the latter was usually found acting slowly and feebly, after the functions of the brain had entirely ceased. In the rabbit, the effects of the arsenic on the stomach and intestines were usually less than in carnivorous animals.

The action of arsenic on the system is less simple than that of the majority of vegetable poisons. As it acts on different organs, it occasions different orders of symptoms; and as the affection of one or another organ predominates, so there is some variety in the symptoms produced even in individual animals of the same species.

In animals killed by arsenic, the blood is usually found fluid in the heart and vessels after death; but otherwise all the morbid appearances met with on dissection are confined to the stomach and intestines. As this is the case, and as the affection of these organs occasions remarkable symptoms, it may be right to mention the result of my observations on this subject.

In many cases where death takes place, there is only a very slight degree of inflammation of the alimentary canal: in other cases the inflammation is considerable. It generally begins very soon after the poison is administered, and appears greater or less according to the time which elapses before the animal dies. Under the same circum-

highly probable, that the heart is affected only through the medium of its nerves; but the affection of the heart is so far independent of the affection of the nervous system generally, that the circulation may cease although the functions of the brain are not suspended, and the functions of the brain may be wholly suspended without the circulation being at all disturbed. In proof of the first of these propositions, I may refer to my former experiments on the upas antiar, in which the sensibility of the animal continued to the very instant of death; and respiration, which is under the influence of the brain, continued even after the heart had ceased to act. In proof of the second, I may refer, among many others, to the experiments detailed in the Croonian Lecture for 1810.

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stances, it is less in graminivorous than in carnivorous animals. The inflammation is greatest in the stomach and intestines; but it usually extends also over the whole intestine. I have never observed inflammation of the œsophagus. The inflammation is greater in degree, and more speedy in taking place, when arsenic is applied to a wound, than when it is taken into the stomach. The inflamed parts are in general universally red, at other times they are red only in spots. The principal vessels leading to the stomach and intestines are turgid with blood; but the inflammation is usually confined to the mucous membrane of these viscera, which assumes a florid red colour, becomes soft and pulpy, and is separable without much difficulty from the cellular coat, which has its natural appearance. In some instances there are small spots of extravasated blood on the inner surface of the mucous membrane, or between it and the cellular coat, and this occurs independently of vomiting. I have never, in any of my experiments, found ulceration or sloughing of the stomach or intestine; but if the animal survives for a certain length of time, after the inflammation has begun, it is reasonable to conclude that it may terminate in one or other of these ways.

I am disposed to believe that sloughing is very seldom, if ever, the direct consequence of the application of arsenic to the stomach or intestines. Arsenic applied to an ulcer will occasion a slough: but its action in doing this is very slow. When I have applied the white oxide of arsenic to a wound, though the animal has sometimes lived three or four hours afterwards, and though violent inflammation has taken place in the stomach and intestines, I have never seen any preternatural appearance in the part to which it was applied, except a slight effusion of serum into the cellular membrane. Arsenic speedily produces a very copious secretion of mucus and watery fluid from the stomach and intestines, which separates it from actual contact with the inner surface of these organs, even though taken in large quantity and in substance; and in animals which are capable of vomiting, by much the greater part is rejected from the stomach very soon after it has been taken in. Hence, though a few particles of arsenic are sometimes found entangled in the mucus, or in the coagulum of extravasated blood, and adhering to the inner surface of the stomach, I have never seen it in such a quantity as might be supposed capable of producing a slough. In one instance, where a dog had swallowed a large quantity of arsenic in substance, a brown spot, about an inch in diameter,

meter, was observed after death on the inner surface of the cardiac extremity of the stomach, having so much of the appearance of a slough, that at first I had no doubt of it being so; but on examination this proved to be only a thin layer of dark-coloured coagulum of blood, adhering very firmly to the surface of the mucous membrane, and having a few particles of arsenic entangled in it. On removing this, the mucous membrane still appeared of a dark colour; but this was also found to arise from a thin layer of coagulum of blood between it and the cellular coat. The mucous membrane itself was inflamed; but otherwise in a natural state. I have observed a similar appearance, but occupying a less extent of surface, several times. In the Hunterian Museum there is a human stomach, which was preserved to show what was considered as a slough produced by the action of arsenic. On examining this preparation, I found that the dark-coloured spot, which had been supposed to be a slough, was precisely of the same nature with that just described.

Although the affection of the stomach and intestines from arsenic is not the cause of death, under ordinary circumstances, it is reasonable to conclude that it may be so in some instances, if the animal survives the effects produced on the organs more immediately necessary to life. Mr. Henry Earle informed me of an instance in which this appeared to be the case. A woman in St. Bartholomew's hospital, who had taken arsenic, recovered of the immediate symptoms, but died at the end of four or five days. On examination after death, extensive ulcerations were found of the mucous membrane of the stomach and intestines, which we can hardly doubt to have been the cause of death.

It is an important matter of inquiry, as connected with judicial medicine, how far may the examination of the body, after death, enable us to decide, whether an animal has died of the effects of arsenic? On this subject, however, I have only a few remarks to make.

The inflammation from arsenic, occupying in general the whole of the stomach and intestine, is more extensive than that from any other poison with which I am acquainted. It does not affect the pharynx or œsophagus, and this circumstance distinguishes it from the inflammation which is occasioned by the actual contact of irritating applications.

But little in general is to be learnt from the examination of the contents of the stomach after death. When arsenic has been taken in substance, small particles of it are frequently

frequently found entangled in the mucus, or in the extravasated blood; but where this was not the case, I have never known, in an animal that was capable of vomiting, that arsenic could be detected in the contents of the stomach after death, though examined by the most accurate chemical tests. As some substances when taken internally are separated from the blood very soon afterwards with the urine, I thought it probable that arsenic might be separated with the urine also; but Mr. Brande (to whom I am indebted for assistance on this, as well as on many other occasions,) could never detect the smallest trace of arsenic in it.

IV. *Experiments with the Muriate of Barytes.*

When barytes is taken into the stomach, or applied to a wound, it is capable of destroying life; but when in its uncombined state its action is very slow. The muriate of barytes, which is much more soluble than the pure earth, is (probably on this account) a much more active poison.

Experiment 5.—Ten grains of muriate of barytes rubbed very fine, and moistened with two drops of water, were applied to two wounds in the thigh and side of a rabbit. In four minutes he was evidently under the influence of the poison. In a short time he became giddy: then his hind legs were paralysed; and he gradually fell into a state of insensibility, with dilated pupils, and lay, in general motionless, but with occasional convulsions. The pulse beat 150 in a minute, but feeble, and it occasionally intermitted. He was apparently dead in twenty minutes from the application of the poison; but on opening the chest the heart was found still acting, and nearly three minutes elapsed before its action had entirely ceased.

Experiment 6.—An ounce and an half of saturated solution of muriate of barytes was injected into the stomach of a full grown cat, by means of an elastic gum tube. In a few minutes it operated as an emetic. The animal became giddy, afterwards insensible, and lay with dilated pupils, in general motionless, but with occasional convulsions. At the end of sixty-five minutes, from the beginning of the experiment, he was apparently dead; but the heart was still felt through the ribs acting one hundred times in a minute. A tube was introduced into the trachea, and the lungs were inflated about thirty-six times in a minute; but the pulse sunk notwithstanding, and at the end of seven minutes the circulation had entirely ceased.

From these experiments I was led to conclude that the principal action of the muriate of barytes is on the brain; but

but in the first the pulse was feeble and intermitting; in the second, although the artificial respiration was made with the greatest care, the circulation could not be maintained more than a few minutes. These circumstances led me to suspect, that although this poison operates principally on the brain, it operates in some degree on the heart also. Further experiments confirmed this suspicion. In some of them the pulse soon became so feeble, that it could be scarcely felt; and its intermissions were more frequent; but in all cases the heart continued to act after respiration had ceased; and the cessation of the functions of the brain was therefore always the immediate cause of death. When I employed artificial respiration, after death had apparently taken place, I seldom was able to prolong the heart's action beyond a few minutes. In one case only it was maintained for three quarters of an hour. I never by these means succeeded in restoring the animal to life, although the experiments were made with the greatest care and in a warm temperature. In some instances, after the artificial respiration had been kept up for some time, there were signs of the functions of the brain being in some degree restored; but the pulse notwithstanding continued to diminish in strength and frequency, and ultimately ceased. I shall detail one of these experiments, as it serves to illustrate the double action of this poison on the nervous and vascular systems.

Experiment 7.—Some muriate of barytes was applied to a wound in the side of a rabbit. The usual symptoms took place, and at the end of an hour the animal was apparently dead; but the heart still continued to contract. He was placed in a temperature of 80°, and a tube being introduced into the nostril, the lungs were artificially inflated about thirty-six times in a minute.

When the artificial respiration had been maintained for four minutes he appeared to be recovering; he breathed voluntarily one hundred times in a minute, and showed signs of sensibility. The artificial respiration was discontinued. The voluntary respiration continued about nine minutes, when it had ceased, and the animal was again apparently dead; but the pulse continued strong and frequent. The lungs were again artificially inflated. At the end of four minutes the animal once more breathed voluntarily one hundred times in a minute, and repeatedly moved his limbs and eye-lids. The pulse became slower and more feeble.

In a few minutes the voluntary respiration again ceased, and

and the artificial respiration was resumed. The pulse had fallen to one hundred, and was feeble The animal again breathed voluntarily; but he ceased to do so at the end of five minutes. The lungs were inflated as before; but he did not give any sign of life, nor was the pulse felt afterwards. On opening the thorax, the heart was found to have entirely ceased acting.

A probe having been introduced into the spinal marrow, it was found that by means of the Voltaic battery powerful contractions might be excited, not only of the voluntary muscles, but also of the heart and intestines; from which it may be inferred, that the muriate of barytes, like arsenic, affects the circulation by rendering the heart insensible to the stimulus of the blood, and not by destroying altogether the power of muscular contraction.

The muriate of barytes affects the stomach, but in a less degree than arsenic. It operates as an emetic in animals that are capable of vomiting; but sooner when taken internally, than when applied to a wound. In general, but not constantly, there are marks of inflammation of the inner membrane of the stomach, but not of the intestine. In many instances there is a thin layer of dark-coloured coagulum of blood lining the whole inner surface of the stomach and adhering very closely to it, so as to have a good deal of the appearance of a slough; and this is independent of vomiting, as where I met with it, it occurred in rabbits.

The same circumstances, from which it may be inferred that arsenic does not produce its deleterious effects until it has passed into the same circulation, leads to the same conclusion with regard to the muriate of barytes.

V. *On the Effects of the Emetic Tartar.*

The effects of the emetic tartar so much resemble those of arsenic and of muriate of barytes in essential circumstances, that it would be needless to enter into a detail of the individual experiments made with this poison.

When applied to a wound in animals which are capable of vomiting, it usually, but not constantly, operates very speedily as an emetic; otherwise I have found no material difference in the symptoms produced in the different species of animals which I have been in the habit of employing as the subjects of experiment. The symptoms are paralysis, drowsiness, and at last complete insensibility; the pulse becomes feeble; the heart continues to act after apparent death; its action may be maintained by means of artificial respiration; but never for a longer period than a few minutes: so that it appears that this poison acts on the heart as well as on the brain; but that its principal action is on the latter. Both the voluntary and involuntary muscles may be made to contract after death, by means of Voltaic electricity. The stomach sometimes bears the marks of inflammation; but at other times it has its natural appearance. I have never seen any appearance of inflammation of the intestines. The length of time which elapses from the application of the poison to the death of the animal, varies. In some instances it is not more than three quarters of an hour; but in others it is two or three hours, or even longer.

When a solution of emetic tartar was injected into the stomach of a rabbit, the same symptoms took place as when it was applied to a wound.

VI. *On the Effects of the Corrosive Sublimate.*

When this poison is taken internally in very small and repeated doses, it is absorbed into the circulation, and produces on the system those peculiar effects which are produced by other preparations of mercury. If it passes into the circulation in larger quantity, it excites inflammation of some part of the alimentary canal, the termination of which may vary accordingly as it exists in a greater or less degree. When taken in a larger quantity still, it occasions death in a very short space of time. I had found, that if applied to a wounded surface, it produced a slough of the part to which it was applied, without occasioning any affection of the general system. This led me to conclude that the effects of it, taken internally and in a large quantity, depended on its local action on the stomach, and were not connected with the absorption of it into the circulation. The following experiments appear to confirm this opinion.

Experiment 8.—Six grains of corrosive sublimate, dissolved in six drams of distilled water, were injected into the stomach of a rabbit, by means of an elastic gum tube. No immediate symptoms followed the injection; the animal made no expression of pain; but in three minutes he became insensible; was convulsed, and in four minutes and an half, from the time of the injection being made, he died. Tremulous contractions of the voluntary muscles continued for some time afterwards. On opening the thorax, the heart was found to have entirely ceased acting, and the blood in the cavities of the left side was of a scarlet colour. The stomach was much distended. The pyloric and cardiac portions were separated from each other by a strong muscular

cular contraction. The contents of the former were firm and solid, and in every respect resembled the usual contents of the stomach; while those of the cardiac portion consisted of the food of the animal much diluted by fluid; so that the solution, which had been injected, appeared to be confined to the cardiac portion of the stomach, and to be prevented entering the pyloric portion by the muscular contraction in the centre.

In the pyloric portion of the stomach the mucous membrane had its natural appearance; but in the cardiac portion it was of a dark-gray colour, was readily torn and peeled off; and in some parts its texture was completely destroyed, so that it appeared like a pulp, on removing which the muscular and peritoneal coats were exposed.

The repetition of the experiment was attended with similar results. The alteration of the texture of the internal membrane appears to have been occasioned by its being chemically acted on by the corrosive sublimate injected into it. When the injection is made into the stomach of a dead rabbit, precisely the same effects are produced, except that, as the middle contraction is here wanting, the appearances are not confined in the same degree to the cardiac portion.

Experiment 9.—A scruple of corrosive sublimate, dissolved in six drams of distilled water, was injected into the stomach of a full-grown cat. For the first five minutes no symptoms were produced. After this, the poison operated twice as an emetic. The animal appeared restless, and made expression of pain in the abdomen. He gradually became insensible, and lay on one side motionless, with the pupils of the eyes dilated. The respiration was laborious, and the pulse could not be felt. Twenty-five minutes after the poison was injected there was a convulsive action of the voluntary muscles, and death ensued. On opening the thorax immediately afterwards, the heart was seen still contracting, but very feebly.

The stomach was found perfectly empty and contracted. The mucous membrane was every where of a dark-gray colour. It had lost its natural texture, and was readily torn and separated from the muscular coat. The internal membrane of the duodenum had a similar appearance, but in a less degree, for nearly three inches from the pylorus. In the situation of the pylorus, the effects of the poison were less apparent than in any other part.

The particular state of the internal membrane of the stomach, in this experiment as well as in the last, appears to have been occasioned by the chemical action of the poison on it. When I injected a solution of corrosive sublimate into the stomach of a dead cat, and retained it there for a few minutes, a similar alteration of the texture of the internal membrane took place; but it assumed a lighter gray colour. The difference of colour may be explained by the vessels in the one case being empty, and in the other case being distended with blood at the time of the injection being made.

The destruction of the substance of the internal membrane of the stomach, precludes the idea of the poison having been absorbed into the circulation. We must conclude that death was the consequence of the chemical action of the poison on the stomach. This organ, however, is not directly necessary to life, since its functions, under certain circumstances, are suspended for hours, or even for days, without death being produced. Although the stomach was the part primarily affected, the immediate cause of death must be looked for in the cessation of the functions of one or more of those organs whose constant action is necessary to life. From the scarlet colour of the blood in the left side of the heart, in the experiment on the rabbit, we may conclude that the functions of the lungs were not affected; but the affection of the heart and brain is proved by the convulsions, the insensibility, the affection of the pulse in both experiments, and the sudden cessation of the heart's action in the first; and we may therefore be justified in concluding, that the immediate cause of death was in both of these organs. As the effects produced appear to have been independent of absorption, we may presume that the heart, as well as the brain, was acted on through the medium of the nerves.

That a sudden and violent injury of the stomach should be capable of thus speedily proving fatal is not surprising, when we consider the powerful sympathy between it and the organs on which life more immediately depends, and the existence of which many circumstances in disease daily demonstrate to us.

VII.

The facts which have been stated, appear to lead to the following conclusions respecting the action of the mineral poisons which were employed in the foregoing experiments.

1. Arsenic, the emetic tartar, and the muriate of barytes do not produce their deleterious effects until they have passed into the circulation.

2. All

2. All of these poisons occasion disorder of the functions of the heart, brain, and alimentary canal; but they do not all affect these organs in the same relative degree.

3. Arsenic operates on the alimentary canal in a greater degree than either the emetic tartar, or the muriate of barytes. The heart is affected more by arsenic than by the emetic tartar, and more by this last than by the muriate of barytes.

4. The corrosive sublimate, when taken internally in large quantity, occasions death by acting chemically on the mucous membrane of the stomach, so as to destroy its texture; the organs more immediately necessary to life being affected in consequence of their sympathy with the stomach.

In making the comparison between them, we observe that the effects of mineral are less simple than those of the generality of vegetable poisons; and when once an animal is affected by the former, there is much less chance of his recovery than when he is affected by the latter.

XLIX. *On Variations in the Vibrations of Pendulums.* By Ez. Walker, *Esq.*

To Mr. Tilloch.

Sir, A letter from Mr. Thomas Reid, of Edinburgh, published in the Philosophical Journal, No. 152, gives an account of his observations on the effect of the attraction between the weight and the pendulum on the going of clocks.

Mr. Reid observes, that "it will perhaps be thought strange to say, that attraction comes in for a share in those obstacles which stand in the way of good time-keeping. This is what has never been even hinted at before; if it has, I confess it is new to me."

Mr. R. fitted up a clock to go a month or six weeks, which he expected to keep the arc of vibration of the pendulum as nearly constant as possible; but after keeping this arc perfectly for above two weeks, it surprised him to find that it began to diminish its arc, and after a time to increase again. This variation in the arc of vibration he was convinced "must arise from the attraction of the weight for the pendulum."

This variation in the arc of vibration, in clocks which go a month or more, is not a new discovery, for it has been known to me more than thirty years. My clock goes

 a month

a month or five weeks with once winding up; but, to prevent the weight's having any effect upon the pendulum, it is wound up every fortnight.

This effect, however, is not produced by the cause that has been supposed; for the attraction between two bodies of a few pounds weight is too minute to produce any sensible effect upon each other, when one of them is in motion. But the true cause will be easily understood if we reflect, that when the weight has descended as low as the bob, the space in which the pendulum then vibrates is very narrow, and consequently it meets with greater resistance from the air, than when the weight is either above or below that place.

The truth of this hypothesis may be easily proved thus: When the weight has descended as low as the centre of oscillation of the pendulum, let the clock-case door stand open, and in less than half an hour the pendulum will vibrate in its usual arc, if not in a greater.

The cause of this increase of the arc is evident; for, the air being less confined, the pendulum meets with less resistance.

I am, sir,

Your obedient servant,

Lynn, Oct. 9, 1812. E. WALKER.

L. *Mr.* HUME *on the Detection of Arsenic.*

To Mr. Tilloch.

SIR, IN the three papers, which I quoted in my last letter to you, I gave no specific process for detecting arsenic by means of the less soluble alkalies, the *earths*,—at least by those generally known by the term, *alkaline* earths; I shall therefore beg you will indulge me by inserting the following short experiments, in the first number of your excellent Miscellany.

Experiment 1.—One grain of white oxide of arsenic was boiled in ten ounces by measure of distilled water, together with a few grains of carbonate of magnesia. This mixture, being filtered and allowed to cool, proved to be a solution of *arsenite of magnesia;* and was obedient to the silver-test, either when applied in the form of simple nitrate or of ammoniaco-nitrate of silver.

Experiment 2.—The same plan was practised with carbonate of lime in lieu of that of magnesia, and the result was a pure *arsenite of lime.*

Experiment

Experiment 3.—In this case I substituted the carbonate of barytes, and the product was a true neutral *arsenite of barytes.*

As neither of these carbonates is soluble in water alone, there can be no error from an excess; I have, therefore, prescribed a *few grains,* meaning that there should, in all cases, be rather more than the arsenic can take up; and hence, these modifications of my test may be considered of some value, and, in particular circumstances, may possess some advantages over the more soluble alkalies.

From these and some other experiments on this plan, which I have not now leisure to describe, I am inclined to believe, that white oxide of arsenic would decompose *all* the earthy carbonates, forming with their respective bases soluble and, perhaps, crystallizable salts.

The arsenite of barytes, on being properly reduced by evaporation, afforded regular crystals, the precise figure of which I did not notice; and the small quantity I had prepared is not now in my possession, having given it to a scientific friend for examination.

Lest the carbonate of magnesia might contain any portion of the precipitant or sulphate of potash, I took especial care, by repeated washings with boiling distilled water, to render it unexceptionable on this score. To the carbonates of lime and barytes, however, there could be no such objection, as both were *natural* productions,—the first being pure marble finely levigated; and the last was the common native carbonate of barytes, containing oxide of iron to be sure, but in all respects sufficiently pure for the purpose, as the iron could have no material effect.

I have not had time to try the carbonate of strontites, although I can scarcely doubt of the same successful issue, whenever this earth shall be subjected to the same process.

With common pipe-clay I also effected an union, but I did not persevere further than merely to assure myself that arsenic did not exist alone in the solution; and I was unwilling to prepare for my purpose an artificial carbonate of alumine, which, notwithstanding the utmost attention, cannot be readily divested of foreign matters.

The *magnesia* in the solution by the first experiment was readily detected by lime-water, and also by a solution of caustic potash. In the second experiment I found the oxalate of ammonia fully to answer my purpose, by proving the existence of *lime.* As to the third experiment, any of the soluble sulphates, or sulphuric acid alone, will render the *barytes* distinct, and form, I believe, a precipitate rather

 more

more copious than in the other examples; and, should this be the case, one would be inclined to prefer carbonate of barytes to the other earthy carbonates, when we adopt this mode of detecting arsenic, namely, by *native* earthy carbonates.

In respect to arsenite of lime, the oxalate of ammonia alone might, I should think, serve as a sufficient test, and prove the existence of arsenic by first showing that of the lime: else, how could *native* carbonate of lime or marble be decomposed under such circumstances? It will serve, however, as a corroborating evidence of the presence of arsenic, and in no small degree add to our knowledge upon this important subject.

The same observations are applicable to the arsenite of barytes. Here, by means of sulphate of soda, for instance, we can separate the earthy base, which must stand as *one* proof; and by the nitrate, or the ammoniaco-nitrate, of silver, for either will succeed, we can discover the arsenic as *another*, and thus complete the demonstration.

Having anticipated "Dr. Roget's Reply," which is inserted in your last number, by my letter to the Editors of the Medical and Physical Journal*, I shall, for the present, abstain from farther animadversions upon that gentleman's "Case of Recovery from Arsenic," and upon his two "Replies," published in consequence of my observations. I shall trust, therefore, that your readers will do equal justice, by permitting me also to refer them to this letter, before they subscribe their assent to what I still consider as an infringement upon my humble pretensions.

My three letters are before the public; they were published three and two years *before* the presumed discovery of Dr. Marcet; they contained the words *potass, lime, soda,* or *any other alkali;* and if these do not comprehend *ammonia*, and, by a fair induction, the alkaline earths also, I must confess there is an imperfection in our language which even our best writers do not regard.

Although it is confessed, that my discovery of the *nitrate of silver* as the most effectual test for arsenic, did not occur to Drs. Marcet and Roget, or to their friends, till their paper was *in* the press, it is evident that many of my own observations were inserted, with some trifling change indeed in the phraseology, *before* the "Case of Recovery" was quite finished and delivered from the hands of the printer. Dr. Roget's friends are, it is well known, both numerous and of the highest respectability; they are men of the first class

* Medical and Physical Journal for October.

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for science and for genius; I am, therefore, astonished they never had even heard of any one of my three letters; while Dr. Henry, a gentleman of no small fame as a philosopher, and of whom and of whose works they cannot be ignorant, writes thus, in his "Elements of Experimental Chemistry," vol. ii. p. 391—"A new process for detecting arsenic has been proposed by Mr. Hume, of London, in the Philosophical Magazine for May, 1809, vol. xxxiii. The *test* which he has suggested, is the fused *nitrate of silver* or lunar caustic."

In my letter to the Editors of the Medical and Phyical Journal, I have said—"in a future edition of this meritorious work (Dr. Henry's Elements), its eminent author will, no doubt, prefer the *silver*-test to all others, and place it at the top of his list, where it will probably always remain." I have spoken nearly to the same effect in my last letter to you;—that this test, under whatever modification or by whatever alkali we operate, "must now supersede every other test for arsenic, and become the standard to all future operators." I then gave you what I called "*another prescription*" for my test,—another modification of the same *silver*-test. This prescription, however, was laid before you with no other intention than to rescue my test, when ammonia is chosen for the medium, from the very inelegant and crude method projected by Drs. Roget and Marcet;—that of employing *two* glass rods, one of which only is required to be *clean;* and likewise to correct a palpable error,—that of using the nitrate of silver and the ammonia *separately* and *without limitation.*

I remain, sir, your obedient servant,

Long Acre, London, Oct. 14, 1812. JOS. HUME.

LI. *Notices respecting New Books.*

Sir HUMPHRY DAVY's "Elements of Chemical Philosophy."

[Continued from p. 151.]

IN the section on "chemical attraction and the laws of combination and decomposition," the Professor has admitted the only deductions which are of a nature entirely theoretical. As these deductions are likely to perform in future as important a part in chemistry as Bergman's tables of attractions have done, it is necessary (although they are not entirely new) to lay before the reader a more detailed account

account of them, and as far as possible in the very words of the author. After stating the nature of combination as exemplified in combining oil and water by means of a solution of potash, the latter having an attraction or affinity for the former, it is observed that a new substance is formed which differs from both the oil and alkali in taste, smell, colour, and in all its sensible qualities. It is a general character of chemical combination, that it changes the sensible qualities of bodies. "The forms of bodies, or their densities, likewise usually alter; solids become fluids, and solids and fluids gases, and gases are often converted into fluids or solids." Salts, gums, &c. dissolve in water; the consumption of charcoal in our fires depends upon its uniting with a part of the air, with which it forms an invisible elastic fluid: mercury is rendered solid by being heated with half its weight of tin, and a substance of this kind is used for silvering mirrors. "The gas produced by the combustion of charcoal is condensed by another gas procured from quicklime and sal ammoniac, when they are mixed over mercury; and the two invisible elastic fluids form a white saline solid," (carbonat of ammonia used for smelling salts.) Many substances may be made to unite by chemical affinity or attraction: fossil alkali, sand and glass of lead melted together form flint glass; porcelain is formed by heating together mixtures of different earths. "That chemical attraction may be exerted between bodies, it is necessary that they should be brought into apparent contact. Thus, no body will act chemically upon another at any sensible distance. A freedom of motion in the parts of bodies, or a want of cohesion, greatly assists combination; and this circumstance is so marked, that it was formerly considered as a chemical axiom, which is still retained in some elementary books, that bodies cannot act chemically on each other, unless one of them be fluid or aëriform. Such an extensive generalization is, however, incorrect: thus, crystalline muriate of lime and snow, both cooled to 0° Fahrenheit, act upon each other and liquefy; and crystals of oxalic acid and dry lime, treated in the same manner, readily combine. The hardest and the densest bodies, however, undergo chemical changes with the greatest difficulty. Thus the sapphire in its crystallized state is not affected by boiling sulphuric acid; but when in a fine powder, as alumine, it is easily dissolved. Minute division, or solution, or fusion, is necessary in almost all chemical processes." These general and well known facts premised, relative to attraction and combination, the author's author's theory of definite proportion in the combination or decomposition of bodies becomes easily comprehended. "All bodies that differ in their nature, combine with different degrees of force." This is exemplified in the tanning substance being attracted from water by skins; in indigo and other dyes being attracted by vegetable and animal fibres, and new combinations of them effected. In consequence of this principle, one body is capable of separating others from certain of their combinations: from the same circumstances, mutual decompositions of different compounds take place. Hence double affinity, or complex chemical attraction. This leads us to the author's principles of DEFINITE PROPORTIONS.

"7. If one part of pure oxygen gas, and two parts of pure hydrogen gas, in volume, be mixed together in a glass tube, over mercury, furnished with wires for passing the electrical spark through it, and they be inflamed by the electrical spark, the gaseous matter will disappear, and water will result. If two parts of oxygen be employed, and two of hydrogen, one part of oxygen will remain: in whatever proportions they are mixed together, it is found, that one of oxygen always condenses two of hydrogen. It is evident, then, that oxygen and hydrogen combine only in definite proportions, and that the water resulting is always the same in its constitution.

"If a piece of well-burnt charcoal be introduced into a vessel, two-thirds filled with oxygen gas, over mercury; and the mercury be brought to the same level on the inside and on the outside of the jar, and the charcoal be inflamed by a burning glass, there will be at first an expansion; but after the experiment is over, it will be found that the volume of the gas has not perceptibly altered; and if the charcoal has been in sufficient quantity, the whole of the oxygen will be found converted into carbonic acid: now the densities of oxygen gas, and carbonic acid gas, in whatever way they are formed, are always the same; and to each other as 34 to 47 nearly. It is evident, then, that carbonic acid must always contain the same weight of oxygen and charcoal. If there is twice as much oxygen in the vessel as is necessary for the consumption of the charcoal, half of it remains untouched: and if the charcoal is partly unconsumed, still the gas is the same in quality; it always contains by weight 5·7 of charcoal and 15 of oxygen.

There is an inflammable gas, called carbonic oxide, which burns with a blue flame, and which is obtained by igniting together zinc filings and chalk. When two in volume of this

this gas, and one in volume of oxygen, are acted upon by an electric spark, over mercury, they inflame, and there result exactly two volumes of carbonic acid gas; there is no other product, and the weight of the carbonic acid gas exactly equals the weight of the carbonic oxide and the oxygen gas: so it is evident, that the carbonic oxide contains exactly half as much oxygen as carbonic acid; that is, 5.7 of charcoal require 7.5 of oxygen to become carbonic oxide. Again, this is proved by decomposition: if electrical sparks be passed through carbonic acid gas, over mercury, it expands, and part of it is decomposed, two volumes becoming two volumes of carbonic oxide, and one volume of oxygen.

"When the salt, called nitrate of ammonia, is decomposed by heat, an elastic fluid is disengaged, called nitrous oxide; when one volume of this gas is mixed with one volume of hydrogen, and an electric spark is passed through the mixture, inflammation takes place, water is formed, and one volume of elastic matter remains, which is azote. Now, as one volume of hydrogen takes half a volume of oxygen, for its conversion into water, it is evident that this gas, nitrous oxide, must be composed of two in volume of azote, and one in volume of oxygen, condensed into a space equal to two.

"There is a gas produced by the solution of copper in diluted nitric acid. If a little of this gas be passed into a curved glass tube over mercury, and metallic arsenic be sublimed in the gas, it is gradually decomposed. A solid combination of arsenic and oxygen is formed, which is found (if the weight of the azote remaining be compared with that of the nitrous gas) to contain half a volume of oxygen, and half a volume of gas remains, which is azote. So it is evident, that as azote combined with one proportion of oxygen gas, forms nitrous oxide, so combined with two proportions, it forms nitrous gas: and one volume of nitrous gas mixed over water with half a volume of oxygen, is condensed, and forms a solution of nitrous acid gas in water. So that this body must consist of azote with four proportions of oxygen, nitrous oxide being considered as azote with one proportion of oxygen; and the quantities in these bodies are always the same."

The "*mode of calculating the numbers representing the elements*" is thus defined:—"The smallest quantity bearing a definite relation to another quantity, or quantities, is always the datum, whether it is the first, second, third, fourth, or any other added quantity in the combination. Potassium forms two combinations with oxygen: 100 of potassium

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in weight unite with 20·1 of oxygen to form pure potash; and with 57·8 to form the orange oxide of potassium. 20·1, the smallest number, is taken; and as 20·1 : 100 :: 15, the number representing oxygen, to 74·99; or adding the minute fractional part to 75; and 57·8 is nearly 3 times 20. And the difference may be easily explained by supposing that in experiments on the peroxide, it is scarcely possible to convert the whole of the metal into potassium.

"To give another instance in which the datum is taken from the peroxide: The peroxide of lead contains from 3 to 3·5 per cent more oxygen than minium; and the first oxide known, massicot, consists of about 100 of lead to 7·52 of oxygen; minium of 100 to between 10 and 12; and the puce-coloured oxide of 100 of metal to about 15; and the smallest proportion amongst these is 3·76 of oxygen; and 3·76 : 100 :: 15 is to 398, the number representing lead; and massicot is supposed to contain twice this quantity of oxygen. 398 : 30 :: 100 is to 7·53.

"It would be easy to bring forward a great collection of evidences to show, that in all compound gaseous bodies, the quantities of the elements are uniform for each species*, and that, when two gaseous elements combine in more than one proportion, the second or third proportion is always a multiple or a divisor of the first; and the case seems to be analogous with respect to all true chemical compounds,

* That the proportions in compound gases are definite has long been generally acknowledged; but Mr. Higgins is, I believe, the first person who conceived that when gases combined in more than one proportion, all the proportions of the same element were equal; and he founded this idea, which was made public in 1789, on the corpuscular hypothesis, that bodies combine particle with particle, or one with two, or three, or a greater number of particles. Mr. Dalton, about 1802, adopting a similar hypothesis, apparently without the knowledge of what Mr. Higgins had written, extended his views to compounds in general. Mr. Richter seems to have been the first person to show, that in the decomposition of neutral salts by double affinity, the neutral state is preserved; and likewise that, when a metallic salt is decomposed by a metal, all the oxygen and acid is transferred, and the metal only changed, and that the new solution is as neutral as the former one. It had been ascertained, by different experiments, that in certain cases when solids dissolved in gases, the volume is unchanged, and some instances of the combination of gases were known, in which the volumes bore simple ratios to each other, as in nitrous oxide, and water; but M. Gay Lussac is the first philosopher who attempted to generalize on the phænomena, and show that in all cases where gases unite, it is always in simple ratios of volume, 1 to 1, or 1 to 2, or 1 to 3, and that the condensation, if any, is in a simple ratio. His very ingenious ideas on this subject were made known towards the close of 1808. Berzelius, in a work published in 1810, has determined, very correctly, some of the definite proportions of several important compounds. See Higgins's Comparative View. Dalton's New Chemical Philosophy. *Richter Ueber die neuren gegenstande der Chemie*. *Mémoires d'Arcueil*, tom. ii. *Berzelius, Annales de Chimie*, tom. lxvii. Thomson's System of Chemistry, vol. iii.

whether

whether solids or fluids, in which no mechanical mixtures can be suspected, and where no partial decompositions can have taken place.

"Thus, if sulphuric acid be poured into any solution of baryta, the solid precipitate of sulphate of baryta which falls down, is uniform in its nature, and always contains about 34 of acid, and 66 of baryta; and the case is the same with other similar compounds, and with neutral salts in general.

"And if two neutral salts mutually decompose each other, in the interchange of principles, there is never an excess of acid or of basis *, and the resulting compounds are likewise perfectly neutral. Thus, if 100 parts of nitrate of baryta, which contain 41 nitric acid, and 59 baryta, be mixed with 67 of sulphat of potash, which consist of 30 of sulphuric acid, and 37 potash, there will be found 89 of sulphat of baryta, and 78 of nitrate of potash; so that 41 of nitric acid will combine with the 37 of potash, and 30 of sulphuric acid with the 59 of baryta.

"It is evident from these circumstances, that when one body has the power of detaching another from its combinations, it will always detach the same proportion. Thus, from whatever basis baryta attracts sulphuric acid, it will always detach the same quantity; and the same quantity of potash, from whatever acid it precipitates magnesia, will always throw down the same proportion.

"8. In cases when an alkaline substance combines with more than one proportion of acid, the same circumstances seem to occur as in the combinations of gaseous bodies. The proportion is either a multiple or a divisor of the first; this is shown by a very simple experiment, first made by Dr. Wollaston: let a given weight of the salt, called carbonate of potash, be thrown into a tube over mercury, and diluted sulphuric acid, sufficient to cover it, be introduced into the tube, a certain volume of carbonic acid gas will be disengaged; let an equal weight of the salt be heated to redness, when it becomes a subcarbonate, and let this subcarbonate be treated in the same way, it will be found to give off exactly half as much carbonic acid gas.

"9. In the combination of solid and fluid substances which have not yet been decompounded, with gases, and in the union of compound inflammable bodies with each other,

* MM. Gay Lussac and Thenard have lately stated, "that in some mutual decompositions of fluates and muriates, slightly acid solutions become alkaline. *Recherches*, tom. ii. page 28. But such changes must be complicated; and perhaps a minute investigation may show that they are not anomalous.

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and in all mutual decompositions between bodies of this class, similar circumstances appear to occur: thus there are two combinations of mercury with oxygen, the black and the red; and one appears to contain twice as much oxygen as the other. There are two known combinations of iron with oxygen, the black and the red oxide of iron; and the oxygen in the first being considered as 2, that in the second must be considered as 3; that is, 100 parts of iron take 29 parts of oxygen to become the black oxide, and 43·5* to become the red.

"The decompositions of compounds containing oxymuriatic gas, or chlorine gas by water, afford the best and most intelligible instances of double decomposition. If equal volumes of light inflammable air or hydrogen, and chlorine be mixed together, and exposed to day-light, they slowly act upon each other, no condensation takes place, and they form an equal volume of muriatic acid gas; so that muriatic acid gas consists of hydrogen and chlorine in equal volumes; and water, as has been before stated, consists of two parts in volume of hydrogen, and one part in volume of oxygen. Now phosphorus and sulphur, and most of the metals, combine with chlorine, and form peculiar compounds, many of which are decomposed by water, and the results are phosphorus, sulphur, or the metals combined with oxygen, and muriatic acid; and the oxidated compounds formed, are the same as those produced in other ways; and it is evident, that the quantity of hydrogen given to the chlorine to form the acid, must be exactly in the ratio of the oxygen added to the inflammable substance or the metal: thus, phosphorus burnt in chlorine in excess forms a white volatile substance, which I have named phosphoranée. When water is added to this, phosphoric and muriatic acids are formed, and there are no other products.

"10. As in all well known compounds, the proportions of the elements are in certain definite ratios to each other; it is evident, that these ratios may be expressed by numbers; and if one number be employed to denote the smallest quantity in which a body combines, all other quantities of the same body will be multiples of this number; and the smallest proportions in which the undecomposed bodies enter into union being known, the constitution of the compounds they form may be learnt, and the element which

* These results I have obtained very nearly, namely, 28 and 43; and they differ very little from those of Mr. Hassenfratz, Dr. Thomson, and Mr. Berzelius.

unites

unites chemically in the smallest quantity being expressed by unity, all the other elements may be represented by the relations of their quantities to unity.

"Hydrogen gas, or inflammable air, is the substance of which the smallest weights seem to enter into combination; and it appears to exist in no definite compound in less proportion than water. The specific gravity of hydrogen is to that of oxygen as 15 to 1; and as 2 volumes of hydrogen to 1 of oxygen enter into the composition of water, the ratio of the hydrogen in water will be to the oxygen as 2 to 15; and it may be regarded as composed of two proportions of hydrogen and one of oxygen: and the number representing hydrogen will be 1, and that representing oxygen 15.

"The weights of equal volumes of azote and oxygen are to each other nearly as 13 to 15: therefore, supposing the number representing the proportion, in which azote combines, gained from the composition of nitrous oxide, which contains two volumes of azote to one of oxygen, it will be represented by 26; and nitrous oxide will consist of two proportions of azote equal to 26, and one proportion of oxygen equal to 15. Nitrous gas will consist of 1 of azote and 2 of oxygen, 26 and 30. Nitrous acid gas of 1 of azote and 4 of oxygen, 26 and 60.

"Ammonia, which is decomposed by electricity into 3 volumes of hydrogen and 1 volume of azote, will consist of 6 proportions of hydrogen and 1 proportion of azote, or 6 and 26.

"The weight of chlorine or oxymuriatic gas, is to that of hydrogen nearly as 33·5 to 1; and muriatic acid gas consists of equal volumes of these gases, and therefore is composed of 33·5 of chlorine, and 1 of hydrogen;—but 2 of chlorine may be made to combine with one of oxygen in volume; and double proportions of this gas combine to form compounds, which, when decomposed by water, afford compounds containing single proportions of oxygen; so that the ratio of chlorine to oxygen is that of 67 to 15, and the number representing chlorine is correctly stated 67.

"In like manner it is easy to deduce the number representing the other undecompounded bodies; and they will be found to correspond as nearly as can be expected, in whatever way they are obtained. Thus, whether the number representing the proportion in which potassium, the basis of potash, combines, be gained from its combination with oxygen or with chlorine, the result will scarcely differ; for 8 grains of potassium, converted into the compound of chlorine and potassium, I have found gain about 7·1 grains, and

and when converted into potash, they gain a grain and $\frac{6}{10}$; and as 7·1 : 8 :: 67 : 75·4; and as 1·6 : 8 :: 15 : 75, giving the number representing potassium as about 75.

"It is easy to form a series of proportional numbers, by taking $\frac{1}{4}$ of these numbers, on the supposition that water is composed of one proportion of hydrogen and one of oxygen: but in this case the number representing the proportion in which oxygen combines must contain a fraction; and the calculations are much expedited, and the formula rendered more simple, by considering the smallest proportion an integer.

"Mr. Higgins has supposed that water is composed of one particle of oxygen and one of hydrogen, and Mr. Dalton, of an atom of each; but in the doctrine of proportions derived from facts, it is not necessary to consider the combining bodies, either as composed of indivisible particles, or even as always united, one and one, or one and two, or one and three proportions. Cases will hereafter be pointed out, in which the ratios are very different; and at present, as we have no means whatever of judging either of the relative numbers, figures, or weights, of those particles of bodies which are not in contact, our numerical expressions ought to relate only to the results of experiments.

"If it should hereafter be discovered, that any of those substances now considered as undecompounded, consist of other elements, these elements must be represented by some division of their numbers; and should even hydrogen be found a compounded body, it would merely be necessary to multiply all the numbers representing the other elements, by some common number which would admit of a division into proportions, representing the elements of hydrogen; so that no discovery concerning the composition of bodies can interfere with the general law of the definite nature of their combinations."

Such is the Professor's illustration of his theory of definite proportions in the chemical composition of bodies. Its simplicity and comprehensiveness must render it a great relief to the memory, enable the mind to methodize the vast multitude of facts which modern experiments have developed, and contribute to furnish a collateral test of the accuracy of analytical results in general. Facts seem to prove "that two or more proportions of one body attract a single proportion of another body with more energy than one proportion, and that two proportions or more adhere to a single proportion with less energy than one proportion; or, at least, that a second or a third proportion adheres with less energy than the first." But before this theory can be sufficiently established, it is necessary to rebut or refute the opinions of Berthollet, who was the first to apply the law of astronomical or physical to chemical attraction. He has endeavoured to prove that the relations of the force of attraction to quantity are universal, and that consequently no such thing as elective affinities, properly so called, can exist. The powers of bodies to combine, he maintains, always depend on their relative attractions and their acting masses, whatever these may be; and he supposes that in all cases of decomposition, where two bodies act on a third, this third is divided between them in proportion to their relative affinities, and their quantities of matter. If this position were correct, there could be no simple law of definite proportions. According to this opinion, a salt crystallizing in a strong alkaline solution must be strongly alkaline, in a weak one less alkaline, and in an acid solution it must be acid; which is not the fact. Sir Humphry investigates this important question at considerable length, with his usual acuteness and perspicuity. He denies the existence of a general law, that the attractions of bodies for each other are inversely as the quantities that saturate. This proposition of Berthollet he reduces to an absurdity. Thus: "magnesia and ammonia take up more sulphuric acid than equal quantities of potash; therefore Berthollet concludes that magnesia and ammonia have a stronger attraction for acids than potash: yet, potash instantly separates magnesia and ammonia from acids; and though the facility with which ammonia is expelled from a compound may be hypothetically accounted for, by assuming that the ease with which it takes the gaseous state assists its escape; yet magnesia is in an opposite case; and to account for chemical changes by supposing the effects of forms of matter which are about to appear, or powers not in actual existence, such as elasticity or cohesion, is merely the solution of one difficulty by the creation of another. Ammonia, when solid or fluid, should require a new force to render it elastic, and the cohesion, in a compound, can only be regarded as the exertion of the chemical attractions of its elements. The action between the constituents of a compound must be mutual; sulphuric acid, there is every reason to believe, has as much attraction for baryta, as baryta for sulphuric acid; and baryta is the alkaline substance, of which the largest quantity is required to saturate sulphuric acid: therefore, in Berthollet's view, it has the weakest affinity for that acid: but less sulphuric acid saturates this substance than any other

other earthy or alkaline body; therefore, according to Berthollet, sulphuric acid has a stronger affinity for baryta than for any other substance; which is contradictory."

The subject of "electrical attraction and repulsion, and their relations to chemical changes," is discussed very amply by the Professor, and many new facts are brought in contrast with each other; but it is candidly acknowledged that we have as yet no definite ideas of many phænomena in electricity. No theory that has hitherto been devised is at all compatible with the well-known facts: and although the theoretical error of the present age may be that of attempting to simplify every thing too far, and to refer numerous phænomena to one general principle; yet it would appear that some simple law, to which all others are subordinate or subservient, is still a desideratum in the science (if a mass of conflicting observations may be so called) of electricity.

[To be continued.]

Directions for Sailing to and from the East Indies, China, New Holland, Cape of Good Hope, and the interjacent Parts, compiled chiefly from original Journals at the East India-House, and from Journals and Observations made during Twenty-one Years' Experience navigating in those Seas. By James Horsburgh, F.R.S. 2 vols. 4to, above 500 pages in each. Black, Parry and Co.

Independent of the incalculable utility of these volumes, as a "Sailing Directory," to navigators, they furnish to geographers a most valuable repertory of the latitudes, longitudes, bearings, &c. ascertained with mathematical accuracy, of nearly 5000 different places. The author, who is hydrographer to the honourable East-India Company, has evidently bestowed an immense labour on the work; and the Philosophical Transactions as well as the pages of this Magazine bear testimony to his talents, industry, and accurate research. To philosophers engaged in tracing the general laws of the winds and tides, this work will be equally interesting, as containing an unequalled collection of facts and observations made by practical men, whose minds were unbiassed by preconceived theories. Projectors of maps, compilers of gazetteers, and all persons interested in the physical geography of the vast portion of the world mentioned in the title, may derive important information from these volumes, which are very judiciously arranged to facilitate reference, and printed with side notes, which lead the reader at once to whatever part he may wish to explore.

plore. In a word, the same philosophical spirit which guided the author in his barometrical observations made in low latitudes and tropical regions, as recorded in the Philosophical Transactions, appears in this valuable Directory.

LII. *Proceedings of Learned Societies.*

LONDON PHILOSOPHICAL SOCIETY.

[Continued from p. 235.]

During last month, the Rev. J. Nightingale, one of the curators, delivered before the Society a lecture on Stenography. He began by remarking, that to those who are wholly ignorant of the art, he feared his best efforts would prove dull and uninteresting; but that if he should fail to amuse and instruct, he begged the Society would not attribute that failure to any real want of interest in the subject itself; adding, that it would be his endeavour to excite in the members, a desire for an art which the lecturer knew they would derive when its merits should be duly appreciated.

"Nor," said the lecturer, "is this method of recommending the art of short-hand writing chargeable with novelty. The well-known Mr. Byrom gave lectures on this subject before some of the most eminent and celebrated men of his day. The Rt. Hon. the Earl of Morton, P.R.S.; the Lord Chancellor Pratt; the well-known Earl of Chesterfield; His Grace the Duke of Queensberry; Dr. John Taylor, chancellor of Lincoln; and numerous members of parliament, were among his auditors and pupils."

After this prefatory apology, the lecturer proceeded to trace the origin and history of the art; commencing with a view of the Rabbinical abbreviations, of which the learned Buxtorf has written an interesting history. It was the opinion of Nicolai that this art was introduced among the Greeks by Xenophon; and this opinion is confirmed by Laërtius, who particularly notices two modes of short-writing, by contracted words, and arbitrary characters or symbols.

The Romans practised the art of short-writing at a very early period; and Ennius the poet is mentioned as the person who first invented a system of writing by which the scribes, or notarii, were enabled to follow the most rapid speakers. This method was improved upon by Tyro, Cicero's freed-man; and still more by Seneca. Suetonius, speaking

speaking of Caligula, expresses his surprise that an emperor in many other respects so expert, should be ignorant of short-hand; and Titus Vespasian is mentioned as being remarkably attached to the art. The lecturer then remarked, that the various modes of short-writing formerly used were composed for the most part of a set of arbitrary marks and characters known only to the persons who invented and used them. The art was consequently much neglected. This neglect is apparent from the book of short-hand mentioned by Trithemius. It was a stenographical dictionary belonging to an abbot, who gladly sold it for a few pence, as the monks of his fraternity had ordered the characters to be erased for the sake of the parchment on which they were written. Some other books of short-hand are mentioned as meeting with a similar fate. There is, however, said the lecturer, a short-hand psalter in the library of St. Germain's at Paris, carefully preserved as a stenographical curiosity. Numerous other specimens were mentioned; and several commendatory epigrams were alluded to from Ausonius, Martial, Mani lius, and others. One of the most ancient methods of short-writing still extant, is a Latin MS. entitled "Ars Scribendi Characteris," which was printed about the year 1412.

Ledwich's "Antiquities of Ireland" contains a specimen of stenographic writing among the Irish, called Babeloth. It has a curious resemblance to some methods now practised. The state of stenography in France, both before and since the revolution, the lecturer described principally by a reference to the Researches upon Printing, by M. Lambinet. He then described the attempt of the late Bishop Wilkins to establish an universal language, and also alluded to the curious writing of the ancient Welch, as given by the ingenious Mr. Owen.

The history of the art in this country commences with the treatise of Dr. Timothy Bright, who published his "Characterie" in 1588. From this period the lecturer proceeded to trace the art in a regular chronological order to the invention of the late Mr., or, as he is commonly called, Dr. Byrom, who introduced a new taste and method in short-writing, which, as the present Dr. Mavor observes, must form the basis of every future rational system.

Having descanted on the various methods in use prior to Byrom's invention, as also on some later schemes, Mr N. proceeded to describe, by appropriate diagrams, figures, and

 letters,

letters, the peculiar merits and character of Byrom's plan; observing as he passed along, that being himself neither an inventor, a publisher, nor a teacher, otherwise than *con amore*, of short-hand, he could have no partial or local interests to serve in the recommendation of this method as superior to every other plan hitherto invented.

On a large sheet the lecturer had delineated the various powers and simple combinations of the straight line and the circle; showing that from these two figures every needful character might easily be derived; and that, in most cases, by simply changing the position of the same figure.

Another sheet, No. 2, contained an easy and apt delineation of the use of the vowels; and this was further illustrated by sheet No. 3.

From several curious mathematical diagrams, the lecturer demonstrated on geometrical principles the true proportion and curvature of the short-hand characters, from the size of the loop, chiefly as they are separately made; and also fixed the proper angle of inclination of certain oblique characters, both curved and rectilinear.

Having described, as distinctly as the nature of the subject and the character of a popular lecture would admit, the elementary principles of Byrom's method, the lecturer next proceeded to recommend the system and the art in general, by the exhibition, round the Society's Hall, of a great variety of short-hand specimens, exhibiting perhaps some of the most curious and interesting examples of the beauty, legibility, and practical brevity of the art ever produced. Among these specimens*, there is one marked C, which may fairly be considered as one of the most curious specimens of short-writing extant. It is a correct fac-simile of Dr. Byrom's own hand-writing, from an original MS. in the possession of the Doctor's niece, a lady of distinguished merit and excellence, at Bageley in Cheshire. This specimen is rendered more curious by an ornamental head-piece, containing an exact sketch of Byrom himself, taken from an original painting; being the only likeness of the Doctor in existence. The words of the specimen are St. Pachomius's soliloquy to the various members of his body, exhorting them respectively to perform their various duties with cheerfulness, obedience, and promptitude.

This interesting lecture was concluded by a recommendation of Byrom's method to the study of the learned pro-

* These specimens, and the various diagrams, &c. &c. are given by the lecturer to the Society; and they are highly deserving of being preserved as curious and valuable exemplifications of this useful art.

fessions;

fessions; to public reporters, to literary men in general, and particularly to the numerous and respectable members of the London Philosophical Society. At the same time the lecturer guarded his auditory against the common delusions of many short-hand teachers, who pretend to convey a perfect knowledge of the art to their pupils in a few lessons, a few hours, or days. There is not any system hitherto invented by which a learner may make himself practically acquainted with the art in so short a time; but if any system yet published could do this, Byrom's is certainly that system—especially when that method is practised with the latest improvements by Mr. Molineux, of Macclesfield.

This analysis of Mr. N.'s lecture on stenography will convey but a very imperfect idea of his various illustrations and reasonings concerning this pleasing and valuable art. It is a subject certainly but ill-calculated to call forth the powers of the orator, or the attractive graces of eloquence: but the Society will be benefited by the attempt; and as the various documents and specimens are deposited in the archives of the Society, they may be referred to on any future occasion, as requisite auxiliaries in the study of Mr. Byrom's Universal English Stenography.

October.—The 5th of this month, being the anniversary of the Society, the members proceeded to ballot for the Officers and Council for the year ensuing; when the following gentlemen were returned, viz.

J. C. Lettsom, M. and LL.D. F.R.S. &c. &c. President.

Admiral Savage.
J. Taunton, F.A.S.
J. T. B. Beaumont, F.A.S.
G. Rees, M.D. } Vice Presidents.

T. J. Pettigrew, Treasurer and Secretary.

Rev. J. Nightingale,
James Andrewes, Esq. } Curators.

J. Miers, Registrar.

Council: J. Adams, M.D. F.L.S.; J. Sowerby, F.L.S.; W. Bullock, F.L.S.; J. Parkinson, F.L.S.; E. Clarkson; T. Bedder; R. Thompson; J. Hare; W. Henley; B. Clarkson; E. Scargill; T. K. Cromwell; J. Hare, jun.; J. Smither; H. Harper; J. B. Brown; W. Reid; R. Hone.

After the ballot, Mr. Pettigrew delivered an oration to the members and their friends, in which he took a view of the utility of literary institutions, and the necessity of cultivating our mental powers. He then gave an account of the origin and progress of the Institution, which afforded flattering prospects of future success, and excited peculiar interest. It now consists of above a hundred members, many of whom are well known literary characters;—the funds are in a very promising state, the library is increasing, and the zeal manifested by the members must render the Society permanent *.

It is the intention of the Society to hold a *converzatione* during the winter months, which will afford the members an opportunity of discussing those subjects which cannot so well be done at the general and more public meetings.

The Society after the oration adjourned, and partook of an elegant dinner, which was well attended.

IMPERIAL INSTITUTE OF FRANCE.

[Continued from p. 157.]

On the 11th of August 1811, M. Arago communicated to the class a memoir, the principal object of which was to show that the luminous rays received, in their passage through several diaphanous bodies, new and peculiar modifications.

It will be recollected that, according to the experiments of M. Malus, the character which distinguishes the direct ray from that which has been polarized in its reflection upon a diaphanous body, is owing to the former dividing itself constantly into two fasciculi in its passage through a rhomboid of calcareous spar; whereas the latter undergoes, in certain circumstances, but a single refraction. If, before submitting the polarized ray to the analysis of a prism of calcareous spar, we make it traverse either a plate of mica or of sulphate of lime, or certain plates of rock crystal, we find that the emergent ray no longer resembles either a direct ray of light or a polarized ray. This new ray will be distinguished from the polarized light, by its always giving two images; and from the direct light, by the property which it has of always dividing itself into two fasciculi, the colours of which are different and complementary.

If a direct ray falls on a glass mirror, under an angle of $35\frac{1}{2}$, and if without changing this inclination we turn the mirror round the ray, the quantity of light which is reflected, and that which is refracted, will be constantly the same. It results on the contrary, from the experiments of M. Malus, that when the mirror receives under the same

* We forbear entering more fully into Mr. Pettigrew's elegant oration, as we understand that the Society intends to publish it.

inclination

inclination rays previously polarized, we find two positions diametrically opposite, in which not a single luminous molecule is reflected. Lastly, if we suppose that, the circumstances being the same, the glass mirror is lighted by rays already modified, by a convenient plate of rock crystal, for example, we shall see it successively tinged at each demi-revolution with the whole series of prismatic colours, as well by transmission as by reflection—with this particularity, that at the same instant these two classes of colours will be different.

A ray of light which traverses two crystals, the principal sections of which are parallel or perpendicular, furnishes only two emergent rays. If a third crystal is interposed between the two first, we see at least four images, except in the case where the principal section of this new crystal is perpendicular or parallel to the principal section of one of the two others: hence results a very simple method of ascertaining when a substance, whatever may be its external form in other respects, has the double refraction: for this purpose, we have only to make it turn between two rhomboids properly placed, and to observe if in certain positions we see four images: now this is what happens, for instance, with plates of mica, as M. Arago ascertained; so that this substance ought to be added to the list of those in which mineralogists have recognised the property of doubling images. M. Arago thinks however, that we cannot rely entirely on the method in question, in order to ascertain when a body has the double refraction; for, among other consequences, it will result from his experiments, that flint glass ought to be admitted into the class of crystallized bodies which double images, since certain plates of this glass, interposed between the two rhomboids, act as if they had an extraordinary plan of refraction, but of which the direction is different in different parts of the plate. It is perhaps right to remark, that M. Arago has ascertained this property in pieces of flint glass more than half an inch thick, and through which no double image was seen, although the prismatic form was given to them; nay, he has sometimes, although rarely, met with fragments of the same glass, which as well as mica, sulphate of lime, &c. act variously upon rays of different colours; but these properties seem to arise from some peculiar circumstances in the fusion of flint glass, for the fragments which possess them are not numerous.

When we interpose a plate of mica of a certain thickness, between two rhomboids the principal sections of which

which are parallel or perpendicular, we see, at every fourth part of the revolution of the mica, the secondary images to which this movement gives rise, acquire an intensity equal to those preserved by the first images: if the plate interposed becomes thinner, the number of rays which it depolarizes also becomes smaller; so that, when a certain term is passed, it acts like a common glass mirror: the singular consequence to which this experiment seems to lead, is, that while the very thin plates of mica have not the double refraction, the plate which results from their superposition enjoys this property in a very marked manner.

Taking advantage of the properties which he had discovered in reflected light, M. Malus contrived a very simple apparatus, which is now used in the workshops where prismatic micrometers are constructed, and by the help of which we may determine the axis of refraction and of crystallization of bodies, and, whatever they may be in other respects, the alterations which they have undergone in their external forms. In communicating the description of his instrument to the Class on the 19th of last August M. Malus announced that he had applied it to the examination of diaphanous mineral substances, and of various chemical products susceptible of crystallization, and had ascertained that all these substances are endowed with double refraction, except those which crystallize in the form of a cube, or regular octahedron, by placing in similar circumstances the fibrous and transparent parts of leaves and flowers, silk, wool, or white hairs, fish scales, horn, ivory, quills, whalebone, &c. M. Malus further ascertained that all these substances modify the lights in the same way as crystallized bodies, so that all of them have an axis of refraction, or of crystallization, as if they were formed of molecules of a determinate form arranged symmetrically with respect to each other.

In detailing these experiments, which are as new as they are interesting, we have endeavoured to give them with the greatest fidelity, and in the precise terms used by the authors. We shall add to them no observations of our own: they are undoubtedly worthy, however, of the notice of experimental philosophers, who by repeating them will probably discover new phænomena, or new applications of those already known, and thus throw light on some intricate questions with which astronomers are perplexed.

[To be continued.]

LIII. *In-*

LIII. *Intelligence and Miscellaneous Articles.*

Mr. Andrew Horn will immediately put to the press a short Essay, in which the seat of vision is determined; and, by the discovery of a new function in the organ, a foundation laid for explaining its mechanism, and the various phænomena, on principles hitherto unattempted.

A physician at Moscow, named Renman, has discovered that the rind of the pomegranate may supply the place of the quinquina in many cases, especially in intermittent fevers. He has published at Moscow a memoir, in which he endeavours to prove the efficacy of his new remedy.

METEOROLITES.

Mr. Sowerby, author of British Mineralogy, has just published a plate representing the meteor-stone which was seen to fall in Yorkshire on the 13th of December 1795, accompanied by engravings of part of the one which fell in Scotland in 1804, and of that which fell in Ireland in 1810, all of which are deposited in his museum.

FALL OF AEROLITES NEAR TOULOUSE, IN FRANCE.

The following relation was lately laid before the Imperial Institute by Senator Chaptal:—" On the 10th April, 1812, at six minutes past eight in the evening, the night being very dark, the atmosphere was on a sudden illuminated by a whitish light, sufficient to see to read by, which lasted about 15 seconds, and disappeared gradually. Two minutes and a half afterwards a considerable detonation was heard, resembling the explosion of a mine, and followed by a commotion so strong that several persons thought it was an earthquake. At Gailloe and at Alby it was supposed that the powder magazine at Toulouse had blown up. Some minutes after this explosion, the sky cleared up, and the stars appeared. Two days afterwards it was known at Toulouse that meteoric stones had fallen six leagues from that city, in the commune of Burgau, in the department of the Upper Garonne, and in that of Savenens, department of Tarn and Garonne. According to the account of M. Filhol, a distinguished physician at Grenade, near Burgau, and that of the curate of Savenens, it appears that a great brightness was seen, like that of a rocket, and a number of explosions heard like a rolling fire of musquetry, which lasted several

several minutes, gradually died away, and was followed by a confused noise from the north-west. Soon after was heard a whistling of bodies passing through the air, like stones thrown from a sling; the detonation and rolling noise was from the south-west to the north-east. Several of these aërolites fell at Pechmeja, at a farm on the side of a wood; one of them upon the house, breaking through the tiles, and bending the laths that supported them. Another fell on the threshing floor, and was picked up by the farmer. Another fell by the side of Gourdas, and several on the side of Seucourien, and one at La Praderes, near Savenens. The utmost distance between the places where they were observed to fall was 4000 toises (about four and a half English miles).—The different specimens brought to Toulouse weighed from six to eight ounces. They are not whole, and have all of them a part of their surface of a blackish colour, and, as it were, carbonaceous. In the interior they are gray, and resemble the stones that fell at Aigle, but appear to contain a much greater quantity of metallic substance. Their specific gravity is 3813. The number of these stones seems to have been very considerable; but the darkness of the night, and the alarm of the spectators, probably prevented many of them from being found."

M. Caron, an eminent French surgeon, and author of several valuable treatises on Croup, has offered a prize of 1000 franks for the best answer to the following question: "Is it probable that tracheotomy can cure the croup?" or, "What are the symptoms which require this operation in the treatment of Croup?" M. Caron's residence is at No. 7, Rue Hyacinthe, à Paris, where communications may be addressed.

Two chemists of Paris have recently made two curious specimens of prepared writing paper, of which the following are the processes:

1. Take gall nuts and sulphate of iron (copperas) well pulverized; rub them dry on paper which is not smooth or hotpressed. The paper will assume a grayish tinge, owing to the powder which is attached to it, and which will adhere sufficiently to bear folding, &c. In order to trace characters on this paper, it is only necessary to use a pen dipped in water, or in the mouth, or even a pointed stick, and the characters will become black and legible.

The second process is described as differing from the first, in in so much as the paper is washed in the materials of which ink is made, and then dried. It is of a yellowish colour and the characters are written on it in the same way. Paper books, or albums, of this description, are now manufactured in great abundance at Paris, and they are in considerable request.

A Mr. Robertson, whose name has been already before the public as an aërial traveller in Denmark, has invented a speaking automaton, which he is now exhibiting in Paris. It already articulates distinctly, in French, the words 'Papa,' 'Mamma,' and 'Long live the emperor Napoleon!' and its powers of language are described as daily increasing. Our readers will probably recollect the flute-player invented by Vaucanson, and the chess-player of Kemper; but we believe the above is the first instance upon record of a speaking figure, if we except the famous head invented by Albertus Magnus, according to the French tradition (but Roger Bacon, according to the English), and which is said to have been broken to pieces by St. Thomas Aquinas.

The members of the Institution lately formed in the Ionian Islands, for the promotion and encouragement of literature, under the title of The Ionian Academy, desirous of information as to the state of civilization in Greece since the downfall of the empire of the East to the present time, have proposed the following questions to the learned throughout Europe. [The memoirs are to be addressed to the Secretary of the Ionian Academy under cover to the English or French *Chargé d'Affaires* at Constantinople.]

1. What schools, libraries, and other institutions for the encouragement of learning were founded in the different provinces of Greece, between the period of the downfall of the Empire of the East (1453) and the present time?

2. What were the establishments for public education founded by the Greeks out of Greece, for the education of their countrymen?

3. Were the printing-houses at Moscopolis, Jassi, and Bucharest, the only establishments of the kind in Greece? Is it true that there was a printing establishment in the Fanal of Constantinople? How long did that continue in existence, which was begun in the Patriarchat at Constantinople during the war between France and Turkey?

4. A biographical notice of the life and works of the learned Greeks who have flourished since the fall of the Empire of the East to the present time.

MR. SAD-

MR. SADLER'S AERIAL VOYAGE.

Mr. Sadler ascended from Belvidere-house, near Dublin, October 1, at 1 P. M. with the wind at south-west, and in 35 minutes had sight of the mountains in Wales: he continued in the same direction till three o'clock, when being nearly over the Isle of Man, the wind blowing fresh, he found himself fast approaching the Welch coast, and at four o'clock he had a distinct view of the Skerry Lighthouse, and the prospect of consummating his ardent hopes of a speedy arrival in Liverpool. The wind now shifting, he was again taken off, and lost sight of land; when, after hovering about for a long time, he discovered five vessels beating down Channel; and in hopes of their assistance, he determined on descending with all possible expedition, and precipitated himself into the sea. In this most critical situation, he had the mortification to find the vessels took no notice of him: obliged, therefore, to re-ascend, he now threw out a quantity of ballast, and quickly regained his situation in the air, to look out for more friendly aid. It was a length of time before he had the satisfaction of discovering any, and then observed a vessel, which gave him to understand, by signal, that she intended to assist him, but could not reach him. Two others also now appeared in sight, and one of them tacking about, hoisted the Manx colours. Night now coming on, he was determined to avail himself of their friendly aid, and once more descended into the sea; but here the wind acting upon the balloon as it lay upon the water, drew the car with so much velocity that the vessel could not overtake it; and notwithstanding he used his utmost efforts, and latterly tied his clothes to the grappling iron, and sunk them to keep him steady, still the balloon was carried away so fast, that he was under the necessity of expelling the gas: upon that escaping, the car actually sunk, and he had now nothing but the netting to cling to. His perilous situation, and the fear of getting entangled, deterred the men from coming near him; until, being in danger of drowning, Mr. Sadler begged they would run their bowsprit through the balloon, and expel the remaining gas. Having done this, they threw out a line, which he wound round his arm, and was then dragged a considerable way before they could get him on board, quite exhausted.

The ship was a herring fisher, from Douglas, in the Isle of Man, called the Victory, commanded by John Lee.

In this situation he was conveyed to Liverpool, where finding the crowd of spectators immense, his clothes being also wet, and in great disorder, he paid a visit on board the Princessa frigate, where he was most politely received, and obligingly accommodated with clothes, by Lieut. Roche.

Saturday, October 3, Mr. Sadler arrived at Holyhead, at three o'clock in the afternoon, on his way to Dublin; and requested Mr. Fellowes (the Agent of His Majesty's packets) would transmit this hasty sketch of his proceedings to his friends in London:

"Holyhead, Oct. 4.

"SIR,—Mr. Sadler arrived here yesterday evening, and shortly after embarked on board the packet for Dublin. I am happy to say, that notwithstanding the great fatigue and anxiety he has undergone, he was in good health and spirits, and intends once more making an attempt to cross from Dublin, when the moon is sufficiently old to enable him to see his way. The approach of night, and fearing he might be involved in darkness on his nearing the coast, induced him to descend at sea. The extraordinary circumstance of not receiving assistance from the first vessels he discovered, when he first descended, is only to be accounted for by the terror his appearance must have occasioned to people not used, or, perhaps, not expecting such a sight: one can hardly conceive it could have been from any other cause, or the want of humanity in their crews.

"Mr. Sadler will furnish you with a more detailed account when he has more time, and it will, no doubt, be a very interesting one.

"I remain, sir,

"Your most obedient servant,

"J. FELLOWES,

"Agent of His Majesty's Packets."

LIST OF PATENTS FOR NEW INVENTIONS.

To William Parker, of Whitechapel, in the county of Middlesex, colourman, for an improvement in the making or manufacturing of great paint.—10th August, 1812.

To James Goodman, of the town of Northampton, saddler, for an improved saddle cloth, for preventing the saddle from running forward on a horse.—12th August.

To Jonas Renshaw, of the town and county of the town of Nottingham, linen and woollen draper, for an improved method of making spots in lace or net work.—14th August.

METEORO-

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND,

For October 1812.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Sept. 26	55	64°	57°	30·09	45	Fair
27	57	67	62	·10	40	Fair
28	62	66	55	29·74	0	Rain
29	55	56	55	30·00	27	Cloudy
30	56	63	55	29·85	16	Cloudy
Oct. 1	56	62	54	·77	10	Rain, Thunder
2	48	65	48	·95	42	Fair
3	46	66	50	30·00	40	Fair
4	50	66	50	29·90	40	Fair
5	54	65	57	·60	36	Fair
6	60	64	46	·30	30	Cloudy
7	44	58	52	.39	36	Fair
8	55	62	47	·20	46	Stormy
9	47	57	50	·44	10	Showery
10	50	56	50	·37	0	Rain
11	50	53	45	·35	10	Cloudy
12	44	52	44	·20	0	Rain
13	43	53	46	.05	27	Fair
14	47	52	45	28·85	0	Rain
15	46	54	42	29·16	16	Showery
16	45	55	42	·50	28	Fair
17	40	54	50	·30	0	Rain
18	50	56	50	28·90	0	Rain
19	54	56	50	·57	0	Stormy
20	51	56	45	·93	22	Fair
21	46	52	48	29·68	41	Fair
22	50	56	50	·45	40	Fair
23	49	55	43	·74	36	Fair
24	43	56	48	·92	43	Fair
25	50	55	50	·70	32	Fair
26	47	54	45	·60	42	Fair

N.B. The Barometer's height is taken at one o'clock.

LIV. *Reply to a Letter of Dr.* Marcet *to Dr.* Bostock, *on the Subject of the Alkali in the Animal Fluids.* By George Pearson, *M.D. F.R.S., &c.*

To Mr. Tilloch.

George-Street, Hanover-Square,
October 27, 1812.

Sir, In your Journal for the preceding month I read the letter of Dr. Marcet addressed to his friend Dr. Bostock, in which he offers the evidence of some experiments to prove that the potash which exists in the animal fluids is in the state of muriat (muriate), and that the whole of the uncombined alkali is soda. It appears that Dr. Bostock was of opinion that the supposed uncombined alkali was potash, as I provisionally concluded, and not soda; but on the representation of evidence just mentioned, he has changed his opinion; and therefore has become the vehicle of Dr. Marcet's letter to the public, confiding, he says, that you Mr. Editor will assent that the experiments stated "*must entirely set the question at rest.*" One authority declaring the question to be entirely set at rest, and the other (the author) affirming that every shadow of doubt is now removed, although I was not ready to believe, as I have had occasion to assert, that more than provisional conclusions are likely to be obtained, I at least expected to find some new contravening testimony. This I was prepared to acknowledge; for, reasoning merely from the known facts, I should have felt no humiliation if new evidence indicated adverse conclusions: *Nos non judicis sed indicis personam sustinemus.* (F. Bacon.)

But on examining the evidence which it is asserted has produced conviction, "removed every shadow of doubt," and "set the question at rest," I was unable to perceive any new facts to alter my former conclusions. Hence, I might have replied merely by a counter declaration, and reference to my unanswered experiments and inferences. As however this mode of procedure may be deemed neither decorous to my opponent and the testimony produced of respectable personal authority, nor satisfactory to the public, I respectfully offer the following brief exposition and remarks. The process which Dr. Marcet says authorises his confidence was this:

The saline matters of the serum of blood were procured by evaporation to dryness, incineration, dissolution in water, filtration, evaporation again to dryness, dissolution in acetic acid, dissolution again of the desiccated acetic com-

 pound

pound in alcohol; evaporation of this to dryness, and fusion. The fused mass amounting to about four grains was divided into four parts—*a*, *b*, *c*, *d*.

1. *a*. 1. "Contained abundance of muriatic acid."

2. Dissolved in water and suffered to evaporate spontaneously, it afforded an efflorescent mass of feathery with cubical crystals.

3. Tartaric acid and oxymuriate of platina manifested the presence of potash.

Now, I can only infer from these experiments that a muriate was present, probably either of soda or of potash, or of both. That potash was present combined, but with what substance is quite equivocal; being only a small fractional part of a grain, it might be united to a double salt; although weakly, yet to be no longer deliquescent. It may also be united to muriatic or other acids, especially the sulphuric and carbonic; but here is no evidence of soda in a free state, and even only equivocal evidence of it as united to muriatic acid. If free soda were present, why was not soda-tartrate of potash observed?

2. The portion *b* with sulphuric acid gave sulphate of soda and sulphate of potash. Here the testimony is equivocal, for the sulphate of soda may, and indeed most probably was from the decomposition of muriate of soda by the sulphuric acid, and the sulphate of potash may arise from the decomposition of potash united to some acid, such as carbonic muriatic, &c. united though weakly to the other salts. Hence I perceive no evidence of soda in a free state.

3. The portion *c* with nitric acid afforded rhomboidal crystals and no prismatical crystals. I will not repeat my objections to any conclusions from the form of crystals, especially in such minute portions of matter as a small part of a grain, set forth so fully in a former paper; but it may be right just to remark that this experiment is inconclusive and unsatisfactory: 1. because, if all the crystals were nitrate of soda, then all the saline mass must be soda; or, 2. if only a part was soda, and the rest was muriate, then this must have been decompounded by the nitric acid: but, 3. if this could happen, then the whole of the rhombs might be from the decompounded muriate of soda: 4. if the whole of the crystals were rhombs of nitrate of soda, what became of the cubical crystals of muriate of potash?

4. The portion *d* with oxymuriate of platina gave a precipitate of potash-oxymuriate of platina, and by evaporation, soda muriate of platina. Here the questions naturally

turally occur; 1. What are the proofs of soda-muriate of platina? 2. What are the proofs that soda muriate of platina was from free soda, and not from muriate of soda?

To omit nothing supposed to be favourable to the adverse party, it must be noticed that "the carbonaceous alkaline mass" above spoken of after fusion, did not deliquesce on exposure to even damp air. I never met with such a result, at least with expectorated matters and dropsical fluids; and if no deliquescence took place with the salts of serum, it is not unreasonable to account for it from the very small proportion, probably not one fourth of a grain, or at most half a grain of alkali in the whole mass; and this by fusion might be united to form a compound unknown.

To the inferences of my adversary I also object; that it is assumed without testimony, that alcohol dissolved a large proportion of muriate of potash. It is I believe admitted (but it may be an error) that this menstruum dissolves none at all: but if this be an error, I demand the proof.

2. It was not admitted as I reasoned that acetate of soda is non-deliquescent, and therefore the proof I offered of the alkali being potash from the deliquescent property of the acetate was eagerly seized to expose my ignorance, by exultingly exclaiming that I had committed *a palpable* error. I acknowledged that I had taken for granted, with most chemists, what I subsequently admitted was not a fact: but I am now in a doubtful state of mind with respect to this property: for Professor Berzelius confidently assures me that he found by repeated experiments acetate of soda to be uniformly non-deliquescent; and on observing that in my experiment I had found it otherwise, we agreed that probably the different results were owing to the soda I used containing a proportion, however minute, of potash, and which I could not perceive by tartaric acid; whereas that he used was exempt. If this be true, it will be a stronger proof that the alkali is potash, than the united testimonies to prove that it is soda.

3. Dr. Marcet argues, that from principle it may be inferred that soda and not potash is the impregnating alkali, because the latter attracts muriatic acid more strongly than the former. This is true in the circumstance of simple elective attraction: but any reasoning from this law when more than one menstruum is present and two or more bases, is fallacious; especially when the different substances present are not certainly known: and here I must observe that I have never contemplated potash as existing in an uncombined state in the animal fluids, but in reality in combina-

 tion

tion with a destructible acid, or with animal oxide. This acid from some trials, I was inclined to propose is the malic acid, but I subsequently did not venture to offer it: however, I find from the conversation of Professor Berzelius, now in London, that he coincided with me in an analogous, if not a similar result. "You very nearly," said he, "made a capital discovery, for I have ascertained it is the *lactic acid* in union with the alkali of the animal fluids." I hope the British public will soon be edified by the translation from the Swedish language of the works of this most acute chemist, and as I hear by a most able editor. Hence much light will be afforded, especially in animal chemistry. This fact is however only within the record before us, to refute any *à priori* conclusion from a case of simple elective attraction. I had long considered the case of this kind noticed by my opponents, for it was too glaring to be unnoticed. If reasoning from principle could be depended upon I would argue; that as all animals either immediately or mediately live upon vegetable matter; and as vegetables very generally contain potash combined with acids, or some other things destructible by fire, it is reasonable to conclude that the fluids of animals must be impregnated with potash in such a state of combination.

I know it has been inferred by some able chemists that the potash must be united to muriatic or sulphuric acid, and soda must be united to some weaker acid, such as carbonic, lactic, acetous, malic, &c. agreeably to the assumed law that the stronger menstruum unites with the stronger basis, and the weaker menstruum with the weaker basis: but there are so many exceptions to this rule that I apprehend it cannot rightly be termed a law.

Lastly, in his P.S. Dr. Marcet says he has instituted the processes above examined, on a large quantity, some gallons of bullocks blood, with the same results as on small quantities of animal matter. I believe such evidence is inadmissible; for, if mere general statements of results be received as testimony, much error will be liable to be introduced, as the public in these cases cannot be in possession of the means of repeating the experiments and judging of their accuracy. It is to be regretted that the author did not render his experiments instructive by a due detail: however, if they were a mere repetition of his former ones, the questionable fact would still remain undetermined.

The chemical world may now perhaps be furnished with the means of judging whether or not Dr. Marcet has "removed

moved every shadow of doubt," by legitimate inductive reasoning. My opponent, not content with proofs by experiment, has endeavoured to command assent by a most respectable authority of opinion. But Truth is not the daughter of mere human authority, but of Time producing evidence of sense and of reason.

I beg permission to make a very few remarks, which although justifiable, yet, being personal, will afford but lenten entertainment, and still less instruction to the public.

In making the above authority the vehicle of his letter, my opponent thinks proper to express disapprobation of my mode of controversy, and to more than insinuate I should not have been honoured with further notice, but for the "interference" of his friend. Accordingly, but for this fortunate circumstance the public would not have been instructed by his letter now under examination. This conduct I own, I think, is rather selfish; for a public-spirited man will always make sacrifices for the benefit of the republic. It is however good, that the *interference* overcame the resolution after four months obstinate resistance. The head and front of my offending was, it seems, to the extent of an attempt to be jocular, in which I never meant to inflict any wound of the feelings. It grieves me to find that some of my expressions were misconstrued insidiousness:—*non vulnera fidelia amantis, sed oscula blandosa malignantis*. In the endeavour to expose the inefficiency of the proposed method of investigation, and to honour illustrious chemists, whose successful methods were unworthily disvalued, I preferred the manner of controversy complained of to the alternative—a serious remonstrance. For, as my affectionate friend, the prince of philologists, now no more! was wont to say, "Cantantes minus via lædit."

In conclusion, I would fain hope that, if this warfare must be continued, special care will be employed that nothing be said, or arise, which can reasonably excite painful sensations, in either party. And if it be agreed that our axioms and conclusions are but inductive reasonings, according to known facts, which therefore are liable to be subverted * by the facts being multiplied; whatever be the

* Experientiæ ordo, primo lumen accendit, deinde per lumen iter demonstrat, incipiendo ab experientia ordinata et digesta, atque ex ea educendo axiomata, atque ex axiomatibus constitutis rursus nova experimenta. —F. Baconis *Novum Organum*.

issue, no humiliation ought to be experienced, as the parties will moult no feather.

I have the honour to be, dear sir,
Your most faithful servant,
GEORGE PEARSON.

P. S.—I have no where charged Dr. Marcet in the terms alleged, viz. that he had committed *blunders* in reasoning. I can well spare that word *blunder* from my vocabulary, having little use for it, although by the law of retaliation amply justifiable.

LV. *Of such Portions of a Sphere as have their Attraction expressed by an algebraic Quantity.*

THE measure of the surface of a sphere involving in its expression the length of the circumference of a circle, it has been thought an interesting problem, even so long ago as the time of Pappus, to assign such portions of that surface as admit of an exact quadrature: and this kind of inquiry has been extended by Euler, and some later writers, to the finding such parts of the solidity as may be exactly cubed.

Now as the attraction of a sphere, on a point at its surface, is expressed by the same kind of transcendental quantity as the surface, or the solidity, it seems a problem equally interesting with those above mentioned, to determine such portions as have their *attraction* an algebraic quantity.

Let the circle AEFG, fig. 5. (Plate VIII.) be the base of a hemisphere, AF a diameter, ABCH a curve touching or meeting the circle at A, and having its portions ABC, AHC equal and similar to one another. Conceive a right cylinder to be erected on the base ABCH, and to pass through the surface of the hemisphere; it is required to find the attraction of that portion of the cylinder, that is intercepted by the hemisphere, on a point at A, in the direction AF. Let a line AD, drawn from A to any point D in the base, be called r; the angle DAF, θ; Let $\varphi\,(r, \theta)$ represent a perpendicular to the base at D, and extended to the surface of the hemisphere. Then, if F denote the force of the cylindrical portion in question, it is easy to see that

$$F = 2\iint \frac{\varphi(r, \theta) \cos. \dot{\theta}.\, \dot{r}}{\sqrt{\varphi(r, \theta)^2 + r^2}} \;\ldots\ldots\ldots\ldots\ldots (\alpha);$$

in

in which the fluent, with respect to r, must be taken from $r = o$, to $r =$ such a function of θ, as the nature of the curve ABC, bounding the base of the cylinder, may require; and then the fluent, with respect to θ, must be taken from $\theta = o$, to $\theta = \frac{\pi}{2}$; where $\pi = 3{,}1415$, &c.

Now, if R be the radius of the hemisphere, it is easily perceived that

$$\phi(r,\theta) = \sqrt{R^2 - r^2 \sin.^2\theta - (R - r\cos.\theta)^2} = \sqrt{2Rr\cos.\theta - r^2};$$

whence

$$F = 2\iint \frac{\sqrt{2Rr\cos.\theta - r^2}\, r\cos.\theta.}{\sqrt{2Rr\cos.\theta}} = 2\iint \frac{\sqrt{2R\cos.\theta - r}\,\dot{r}\cos.\theta.\dot{\theta}}{\sqrt{2R\cos.\theta}};$$

by a first integration of which we get

$$F = \frac{4}{3}\int \left\{ \frac{(2R\cos.\theta)^{\frac{3}{2}}}{(2R\cos.\theta)^{\frac{1}{2}}} - \frac{(2R\cos.\theta - r)^{\frac{3}{2}}}{(2R\cos.\theta)^{\frac{1}{2}}} \right\} \cos.\theta.\dot{\theta};$$

$$\text{or,}\quad F = \frac{2}{3}R\pi - \frac{4}{3}\int \frac{(2R\cos.\theta - r)^{\frac{3}{2}}}{(2R\cos.\theta)^{\frac{1}{2}}} \cos.\theta.\dot{\theta} \ldots\ldots (\beta).$$

Here the first term is the attraction of the whole hemisphere; and consequently the other term expresses the action of that part of the hemisphere which remains after taking away the portion cut out by the cylinder; so that the solution of our chief problem will be attained by making this an algebraic quantity. But I shall first show the use of the preceding formula, by seeking the attraction of such a portion as is intercepted by the cylinder in Viviani's celebrated problem, where the base ABCH is a circle. Let the radius of the base be any line ρ less than R, we have, by the nature of the curve, $r = 2\rho\cos.\theta$; so that the term to be integrated is

$$-\frac{4}{3}\int \frac{((R-\rho)\,2\cos.\theta)^{\frac{3}{2}}}{(2R\cos.\theta)^{\frac{1}{2}}} \cos.\theta.\dot{\theta} = -\frac{4}{3}\int (R-\rho)$$

$$\left(\frac{R-\rho}{R}\right)^{\frac{1}{2}} \times 2\cos.^2\theta.\dot{\theta} = -\frac{2}{3}(R-\rho)\left(\frac{R-\rho}{R}\right)^{\frac{1}{2}}\pi; \text{ whence}$$

$F = \frac{2}{3}R\pi - \frac{2}{3}(R-\rho)\left(\frac{R-\rho}{R}\right)^{\frac{1}{2}}\pi$; or, *the attraction sought is the difference of the actions of the hemisphere itself, and of another hemisphere, whose radius is* $(R-\rho)\left(\frac{R-\rho}{R}\right)^{\frac{1}{2}}$.

Let us now go back to equation (β), and if we only consider

sider that portion which remains after penetration by the cylinder, it becomes

$F = \frac{4}{3}\int \frac{(2R \cos.\theta - r)^{\frac{3}{2}}}{(2R \cos.\theta)^{\frac{1}{2}}} \cos.\theta.\dot{\theta}$; and this is to be an algebraic quantity.

1. Suppose the curve ABFH, fig. 6. (Plate VIII.) to be of such a nature, that the radius vector A*m*, or $r = 2R \cos.\theta\,(1 - \sin.^{2n}\theta)$; we get by substitution,

$$F = \frac{4}{3}\int \frac{(2R \cos.\theta.\sin.^{2n}\theta)^{\frac{3}{2}}}{(2R \cos.\theta)^{\frac{1}{2}}} \cos.\theta.\dot{\theta} = \frac{8}{3} R\int \cos.^{2}\theta.$$

$\sin.^{3n}\theta.\dot{\theta} = \frac{8}{3} R\int\left\{\sin.^{3n}\theta.\dot{\theta} - \sin.^{3n+2}\theta.\dot{\theta}\right\}$; which expression will always be an algebraic quantity, when n is an odd positive whole number. If we choose to express the equation of this class of curves by rectangular coordinates, make $An = x$; $mn = y$; then $Am = r = \sqrt{x^2 + y^2}$; and putting these values in the equation $r = 2R \cos.\theta\,(1 - \sin.^{2n}\theta)$, we shall get

$$(x^2 + y^2)^{n+1} = 2Rx\left\{(x^2 + y^2)^{n} - y^{2n}\right\}.$$

In the simplest case, when $n = 1$, this becomes $(x^2 + y^2)^2 = 2Rx^3$; a line of the fourth order.

2. Let $r = 2R \cos.\theta\,(1 - \cos.^{2n}\theta)$, in which case

$$F = \frac{4}{3}\int \frac{(2R \cos.^{2n+1}\theta)^{\frac{3}{2}}}{(2R \cos.\theta)^{\frac{1}{2}}} \cos.\theta.\dot{\theta} = \frac{8}{3} R\int \text{Cos.}^{3n+2}\theta.\dot{\theta}.$$

This, like the former, will be an algebraic expression when n is an odd positive whole number. The base of the cylinder will here have two parts in the manner represented by fig. 7. (Plate VIII.) The equation of these curves may be expressed by rectangular coordinates as in the last case; but having a few words to say on another subject, I shall for the present quit this.

X. Y.

P. S.—In my letter of last month, respecting Mr. Legendre's principle, I agreed with that mathematician, that there can be no equation between c and A, B, C; but then I must be understood to mean, between those quantities *alone*; "sans autre angle ni ligne quelconque;" which is all he professes to prove, and what every child knew before. But, who ever dreamt that in a case of this kind it is necessary there should be an *equation* between the variable quantities

quantities *alone?* The attraction (F) of the earth, on a particle at any distance (y) from its centre, is *entirely determined by y alone:* but there can be no *equation* without the assistance of other quantities.

Let Φ and Υ be any fixed or given values of F and y, we have $F : \Phi :: \frac{1}{y^2} : \frac{1}{\Upsilon^2}$, whence $F = \Phi \times \Upsilon^2 \times \frac{1}{y^2}$. Here then is an *equation;* but between F and y *alone*, we could only have an *analogy;* viz. $F \propto \frac{1}{y^2}$. The case is exactly similar with respect to the side of a triangle, and the three angles. Though there can be no equation between c, A, B, C *alone;* yet, if x, α, β, γ be fixed values of those quantities, there is no reason why we might not have $c : \varphi(A, B, C) :: x : \varphi(\alpha, \beta, \gamma)$; whence

$c = \frac{x}{\varphi(\alpha, \beta, \gamma)} \times \varphi(A, B, C)$; and even if we should grant, to please the Edinburgh Reviewer, that A, B, C are of the nature of numbers, it will not hinder each side of this equation from being of one dimension. But I confess I have scarcely patience to speak *seriously* on a subject which every one but the Reviewer* sneers at: and *he*, I fancy, will not shortly be induced to discuss this topic a third time.

LVI. *On the Differential Thermometer.*

To Mr. Tilloch.

Sir, Mr. Leslie has stated that the curious instrument described by Van Helmont is the same as the air thermometer of Drebbel or Sanctorio. If this was the case, as Van Helmont was of the same age as those men, and as he has not quoted them, but calls the instrument his own, "suum organum," it might be still a question, whether he was not an inventor; but whoever will compare Drebbel's Thermometer as copied in Shaw's Translation of Boerhaave's Chemistry, and Sanctorio's as engraved in his works, fol. Venetiis, pag. 22, with Van Helmont's, will find them wholly unlike. Sanctorio's (fig. 1. Pl. VIII.) and that attributed to Drebbel (fig. 2.) consisted of a single globe and an open tube, and the end of the tube was introduced into a vessel containing coloured liquor. In the figure attached to the first edition of Van Helmont, published after his death in 1648, of which fig. 3. is a copy,

* Anticyram ratio *illi* destinat *omnem.*

there

there is no aperture delineated, and it might be conceived hermetically sealed. Mr. Leslie has introduced a cork, and his figure certainly differs from those in the two editions of Van Helmont which are found in the library of the College of Edinburgh, and which are perfectly alike. Fig. 4. is copied from the edition of 1652.

Mr. Leslie has not noticed a sentence immediately following that to which he has referred, in which the principle of the differential thermometer is described. I shall supply his deficiency: "Applicatur vasi A aliquis calor, excedens temperiem ambientis: tunc enim aër inclusus, se dilatabit, secundum plus aut minus caloris, ac juxta, atque excedit veram aëris temperiem clausi in vase D, contra quem propellens aquam BC destruet æquilibrium per nimiam actionem. Ita quod in D erit compressus et condensatus aër, per restrictionem: ut cedat dilatationi factæ in A."

It appears from these facts that Sir H. Davy is fully justified in saying that a curious instrument like the differential thermometer is described by Van Helmont, "which appears to have been the first in which the expansive power of heated air was exhibited by its action upon cold air."

I shall not pretend to determine whether Mr. Leslie has not read this last passage, or whether he has read it and has not thought proper to quote it: nor shall I offer any observations on insinuations so unwarrantably and unjustly brought forward by the Professor: as the ground for them has been removed, they will fall of course.

I am, sir, yours, &c.

Edinburgh, Oct. 20, 1812. A. B.

LVII. *Tabular Corrections for the Rise and Fall of the Mercury in the common Barometer. By* E. Walker, *Esq.*

To Mr. Tilloch.

Sir, No meteorological instrument is in higher estimation than the Barometer. But barometers of the common construction, with an upright tube and open cistern, are not sufficiently correct for investigating the laws of meteorology with the greatest precision, in consequence of the quicksilver's rising or falling in the cistern with every variation in the pressure of the atmosphere. In the best barometers, however, this imperfection is corrected by some

some mechanical contrivance; but none of these can be added to an instrument, without adding to its price.

But this imperfection may be removed, without any additional apparatus or expense; for we need only find how high the surface of the mercury in the cistern would be raised by any given fall in the tube, to find the correction for every other point in the scale.

Suppose it were required to find the correction for *one* inch rise of the quicksilver above the zero, or that point in the scale where no correction is necessary: Let $a =$ the diameter of the cistern, and $b =$ the inside diameter of the tube; then, as equal cylinders have their altitudes reciprocally as their bases, we have $aa : bb :: 1 : \frac{bb}{aa}$, which is the correction to be added to the altitude of the column of quicksilver, when it rises *one* inch above the zero, and subtracted from the altitude of the column, when its surface falls *one* inch below that point.

Example.—Suppose the inside diameter of the tube be $\frac{2}{10}$ of an inch, and the diameter of the cistern be *two* inches $= \frac{20}{10}$: Then $\frac{4}{400} = \cdot01 =$ the correction for one inch rise or fall of the mercury in the tube.

Suppose the barometer needs no correction, when the surface of the quicksilver stands at 29·5 on the scale, then the corrections for the whole scale, according to the preceding example, would be as in Table 1.

But Mr. De Luc observes, that the cistern barometer does not give the true pressure of the atmosphere; the quicksilver in it being a little depressed on the same principle as in capillary tubes. It is therefore thought necessary, where so much nicety is required, to determine by experiment how much the quicksilver is depressed in tubes of a given bore, and to allow accordingly. By some experiments which have been made on this subject by Lord Charles Cavendish, the depression appears to be ·067 of an inch for a tube whose inside diameter is 0·2.*

Now, if ·067 be added to each of the corrections in Table 1. we shall have the corrections in Table 2.

* Philos. Trans. vol. lxvi. for the year 1776.

TABLE

Table 1.		Table 2.	
Alt. in Inches.	Corrections.	Alt. in Inches.	Corrections.
30·5	+ ·01	30·5	+ ·077
30·	+ ·005	30·	+ ·072
29·5	.. ·000	29·5	+ ·067
29·	— ·005	29·	+ ·062
28·5	— ·01	28·5	+ ·057

But if the inside diameter of the tube be larger at one end than at the other, the corrections cannot be found by the preceding rule; but by the following method they may be obtained, with the same precision as before.

Fill about five or six inches of the sealed end of the tube with quicksilver; and after having determined the altitude of the surface of the mercury in the cistern, pour that which is in the tube into it, and measure how high the surface of the fluid has been raised. Then divide this rise in the cistern by the number of inches of mercury poured into it, from the tube, and the quotient will be the correction for one inch rise of the mercury above the zero.

Suppose the number of inches of qnicksilver in the tube were five, and this quantity raised the surface of the fluid in the cistern 5-100dths of an inch, then the correction would be 1-100dth of an inch, for every inch rise of the mercury above the zero; from whence all the other corrections may be found.

If the numbers found by these rules were engraved upon the plate of the instrument, in some form like Table 2, or written upon a slip of paper and pasted upon the frame, the altitude of the quicksilver in the tube might then be corrected by inspection, and perhaps to as great a degree of precision as by any other method.

I am, sir,

Your obedient servant,

Lynn, Nov. 13, 1812. Ez. Walker.

LVIII. *On Tests for Arsenic.* *By* P. M. Roget, *M.D.*

To Mr. Tilloch.

Sir, It is with reluctance that I again trouble you on a subject in which the public is so little interested, as the controversy respecting the priority of Dr. Marcet or Mr. Hume's

Hume's application of ammonia in the test for the detection of arsenic. But as the latter gentleman still asserts his exclusive claim to the invention, in a way that impeaches the accuracy of my referrences, I beg leave, for the sake of those readers who would not be at the pains to consult the original documents, to correct a material error of which Mr. Hume has been guilty in quoting his own letter. He infers that he originally suggested the employment of ammonia with the nitrate of silver, from his having used the expression "*or any other alkali* *." Now it will be found that in the only instance in which that expression occurs †, it is used with reference to a totally different test; namely, the *sulphate of copper;* and has therefore nothing to do with the subject in dispute, which relates entirely to the employment of ammonia with the *nitrate of silver*.

After this specimen of Mr. Hume's mode of reasoning, I trust it is unnecessary for me to pursue any further a discussion in which I see no prospect of satisfying his expectations, and which must already have exhausted the patience of your readers.

I am, sir,

Your most obedient servant,

Bernard-Street, Russell-Square, Nov. 11, 1812. P. M. ROGET.

LIX. *On the Vibrations of Musical Strings; with a Mode of ascertaining the Sound producible by any given Number of Vibrations.* By Mr. JOHN SOUTHERN.

To Mr. Tilloch.

SIR, MUSIC being a subject which has been pretty much entertained in the Philosophical Magazine, you will probably give a place in it to this letter in one of your early numbers; which will much oblige me.

In reading at various times on this subject, I have observed a considerable difference in the number of vibrations assigned to the note C in the middle of the musical scale. Dr. Smith in his Harmonics says, that D on the organ in Trinity College, Cambridge, made 262 vibrations in a second of time; consequently C must have made 234 or 235;

* His words are these: "The question seems capable of being reduced merely to this; What is comprehended under such expressions as *potass, lime, soda*, or *any other alkali?* Does not this language include *ammonia, barytes, strontites*, and *magnesia* at least?" See the Medical and Physical Journal for October last, (vol. xxviii.) p. 289.

† Medical and Physical Journal, vol. xxviii. p. 448, line 34.

and

and he says it was near the Roman pitch. Dr. Young states the number at the eighth power of 2, or 256. In page 436 of the xxxvith vol. of the Philosophical Magazine, 240 is given as the number, as it is also in the number for July last, on the authority of Dr. Crotch. I have examined two modern forks for this note, and have found one to make $254\frac{2}{3}$ and the other $252\frac{1}{4}$ vibrations in a second: so that it appears there is no precise number of vibrations which has yet been fixed upon as a *standard of musical pitch*. It must be confessed that the number is purely arbitrary; but if that number shall be preferred which is more convenient than another in this kind of calculations, and at the same time approaches nearly to that which corresponds with the present concert pitch, 252 is probably the best, being divisible by all the digits except 5 and 8.

But whatever number may be fixed upon as a standard by those interested in any way in its determination, I am persuaded that the following mode of ascertaining the sound producible by any given number of vibrations will be found very practicable and convenient.

Take a fine steel wire, such as is used for the upper strings of a harpsichord, and while it is stretched by a weight ample enough to straighten it, measure and cut off a yard long of it, and weigh it accurately in grains troy. Suspend this piece by one end from a peg, and to the other hang a weight, equal to as many pounds avoirdupoise as the yard long of the wire weighed grains. If then the wire be made to vibrate, and the length of the vibrating part be limited to 19·573 inches, the number of vibrations in one second will be 252. If any other number of vibrations be wanted to be produced, divide 4932·34 by that number, and the quotient will give the number of inches to which the vibrating part of the wire must be limited to produce it: thus, if 256 were wanted to be produced, the length would be $4932,34 \div 256 = 19{\cdot}267$ inches, &c. &c.

A monochord constructed so as to have its wire nearly vertical, and kept in tension by a weight proportioned as above, would be very applicable to this mode of ascertaining sounds.

I have mentioned a fine steel wire, as being, I think, best for the purpose; but good brass wire will do equally well, and so would catgut if it were not for its stretching property, and the inequality of its diameter. In general it will be found convenient to use a small ring of wire, and extend it in the well of a staircase, so that while a weight is appendant to it five or six yards may be measured, and then cut

cut off and weighed; and thereby the weight of a yard more accurately ascertained.

Dr. Smith in the 2d edit. of his Harmonics, prop. 24. coroll. 1. gives this equation:

$\frac{355}{113}\sqrt{\frac{P}{p} \times \frac{39\cdot126}{AB}}$ = the number of semi-vibrations made in a second; consequently $\frac{355}{2\times113}\sqrt{\frac{P}{p} \times \frac{39\cdot126}{AB}}$ = the number of complete vibrations; wherein P = the weight by which the vibrating string is kept in tension, p = the weight, and AB the length (in inches) of the vibrating part of it.

Let the number of complete vibrations be called n; then, if P be made equal to a given length of the string, as m yards; and if y = the weight of a yard long of it, P will become $= my$, and $p = \frac{AB\times y}{36}$; for AB : p : : 36, (the inches in a yard) : y. Substituting these values of P and p in the equation above in their places, it will become

$$n = \frac{355}{2\times113}\sqrt{\frac{36my}{AB\times y} \times \frac{39\cdot126}{AB}} = \frac{3\cdot1416}{2AB}\sqrt{36m\times39\cdot126},$$

and $AB \times n = \frac{3\cdot1416}{2}\sqrt{36m\times39\cdot126}$.

Now, let m be made = 7000, that is P = as many pounds weight (avoirdupoise) as a yard long of the string weighs grains troy (7000 gr. being equal to a pound): then will $AB\times n = \frac{3\cdot1416}{2}\sqrt{36\times7000\times39\cdot126} = 4932\cdot34$.

In words: If the string be kept in tension by a weight equal to 7000 yards long of the string, then will the length of the vibrating part (in inches) multiplied into the number of vibrations per second, be equal to the constant number 4932·34.

Although Dr. Smith recommends (p. 15. art. 9.) a certain mode of tuning instruments having the imperfect scale, which was then and still continues in common use, by making the major thirds and fifths beat equally quick with their base, which may suffice for organs, in which the beats are so distinct, he has not given a table showing the relative lengths of the monochord, which would give the sounds according to that mode, as he has done at page 224, 224, for those of equal harmony, mean tones, and Mr. Huygens; I therefore subjoin one, and will here show the steps of the calculation.

The Doctor correctly states, p. 219, that the temperament of the 5th, when the beats are to be equally quick with those of the 3d, to the same base, is 5-23ds of a comma. Hence, if from the logarithmic ratio of the perfect 5th 2 : 3 .... 0·1760913

5-23ds of the logarithmic ratio of 80 : 81 (0·0053950) be subtracted	0·0011728
There remains the log. ratio of the tempered 5th	0·1749185
This subtracted from the log. ratio of the perfect octave	0·3010300
Leaves the log. ratio of the tempered 4th ...	0·1261115
The difference between the log. ratios of the tempered 5th and 4th, gives the log. ratio of the tone	0·0488070
Log. ratio of two tones, being the tempered 3d major ....—...................	0·0976140
Which taken from log. ratio of tempered 4th, leaves the limma major, or greater half tone	0·0284975
Which taken from the tone, leaves the limma minor, or the less half tone	0·0203095

These two last logarithms successively subtracted, according to the proper order, from the logarithms 5·0000000 and 1·2916574, and added to 2·4014005, (beginning at the bottom) will give the logarithms found in the 3d, 4th, and 5th columns of the Table; of which those in columns 6th, 7th, and 8th, are respectively the natural numbers. The above logarithms are respectively those of the whole proportional string, of the whole length of the monochord string, and of the number of vibrations in a second of the latter.

I am, sir,

Your most obedient servant,

Soho, in Birmingham,
Nov. 16, 1812.

JOHN SOUTHERN.

TABLE

TABLE.

1. Name of Note.	2. Interval, whether Major or Minor Limma.	3. Log. of proportional Lengths of String, the whole being 100000.	4. Log. of Length of String on the proposed Monochord.	5. Log. of Number of Vibrations of the Monochord.	6. Proporl. Length of String, the whole being 100000.	7. Length of String on the Monochord in Inches.	8. Number of Vibrations of the String in a Second.
C		4·698970	0·990627	2·702431	50000	9·787	504·00
B	+	4·7274675	1·019125	2·673933	53391	10·450	471·99
Bb	−	4·7477770	1·039434	2·653624	55947	10·951	450·43
A	+	4·7762745	1·067932	2·625126	59741	11·693	421·82
G♯	+	4·8047720	1·096429	2·596629	63793	12·486	395·03
G	−	4·8250815	1·116739	2·576319	66847	13·084	376·98
F♯	+	4·8535790	1·145236	2·547822	71380	13·971	353·04
F	−	4·8738885	1·165546	2·527512	74798	14·640	336·91
E	+	4·9023860	1·194043	2·499015	79870	15·633	315·51
Eb	−	4·9226955	1·214353	2·478705	83694	16·381	301·10
D	+	4·9511930	1·242850	2·450208	89370	17·492	281·97
C♯	+	4·9796905	1·271348	2·421710	95431	18·669	264·06
C	−	5·0000000	1·291657	2·401401	100000	19·573	252·00

LX. *Additional Experiments on the Muriatic and Oxymuriatic Acids.* By WILLIAM HENRY, *M.D. F.R.S. V. P. of the Lit. and Phil. Society, and Physician to the Infirmary, at Manchester**.

THE experiments which form the subject of the following pages, are intended as supplementary to a more extensive series, which the Royal Society did me the honour to insert in their Transactions for the year 1800 †. Of the general accuracy of those experiments I have since had no reason to doubt; and their results, indeed, are coincident with those of subsequent writers of the highest authority in chemistry. My attention has been again drawn to the subject

* From the Philosophical Transactions for 1812, part ii.

† Page 188.—Phil. Mag. vol. vii. p. 211. 332.

subject by the important controversy which has lately been carried on between Mr. Murray and Mr. John Davy, respecting the nature of muriatic and oxymuriatic acids *; and I have been induced, by some hints which the discussion has suggested, not only to repeat the principal experiments described in my memoir, but to institute others with the advantage of a more perfect apparatus than I then possessed, and of greater experience in the management of these delicate processes.

This repetition of my former labours has discovered to me an instance in which I have failed in drawing the proper conclusion from facts. In two comparative experiments on the electrization of equal quantities of muriatic acid gas, the one of which was dried by muriate of lime, and the other was in its natural state, I found a difference of not more than one per cent. in the hydrogen evolved, relatively to the original bulk of the gas †. Yet, notwithstanding these results, I have expressed myself inclined to believe that some water is abstracted by that deliquescent salt; and this belief was confirmed several years afterwards, by the event of an experiment in which muriatic acid gas, dried by muriate of lime, gave only $\frac{1}{35}$ its bulk of hydrogen ‡, a proportion much below the usual average. The question, however, was too interesting to be left in any degree of uncertainty; and I have, therefore, made several fresh experiments with the view to its decision. In the course of these I have found, that though differences in the results are produced by causes apparently trivial, some of which I shall afterwards point out, yet that under equal circumstances, precisely the same relative proportion of hydrogen gas is obtained from muriatic acid gas, whether exposed or not to muriate of lime; and that its greatest amount does not exceed $\frac{1}{16}$ or $\frac{1}{14}$ the original volume of the acid gas.

In the paper last quoted §, I have also described an experiment, in which sensible heat was evolved by bringing muriate of lime into contact with muriatic acid gas; a fact which, if established, would go far to prove the existence of water in that gas. But on repeating the experiment with muriate of lime recently cooled from fusion, and over mercury carefully deprived of all moisture by boiling, I was not able to discover any increase of temperature, though a very sensible air thermometer was inclosed in the vessel containing the gas. The evolution of heat takes place,

* Nicholson's Journal, vol. xxviii. and xxix.

† Phil. Trans. p. 191.—Phil. Mag. vol. vii. p. 213. ‡ Phil. Trans. for 1809, p. 433.—Phil. Mag. vol. xxxiv. p. 371. § P. 433, note.—Phil. Mag. vol. xxxiv. p. 371, note.

only

only when the muriate of lime has attracted moisture, either from the atmosphere or the mercury, and is then owing to a condensation of a part of the gas.

Essentially, the changes produced by electrifying muriatic acid over mercury are those which I have stated; viz. a contraction of the volume of the gas, the formation of muriate of mercury (calomel), and the evolution of hydrogen. Recent experiments, also, have confirmed the accuracy of the observation *, that when a certain effect has been produced by electricity, nothing is gained by continuing the process; for neither is more hydrogen evolved, nor can the contraction of bulk be carried any further.

I have lately applied, to experiments on muriatic acid, an apparatus which I used advantageously for the analysis of ammonia †. It consists of a spherical glass vessel, into which are hermetically sealed two small tubes containing platina wires, the points of which approach within the striking distance. To the globular part is attached a neck, which may be closed, as occasion requires, either by a glass stopper or by a metal cap and stop-cock. Into a vessel of this kind I introduced $4\frac{1}{2}$ cubic inches of muriatic acid gas, and passed through it 3000 discharges from a Leyden jar; at the close of the process, no traces of moisture could be perceived on the inner surface of the vessel, nor could I discover, on opening the stopper, that any change of bulk had taken place. After absorbing the unchanged muriatic acid gas by a small quantity of water, a volume of gas remained, in which there were present 100 measures (each equal to one grain of mercury) of oxymuriatic acid gas, and 140 measures of hydrogen. Two causes might, perhaps, contribute to diminish, in some degree, the proportion of the former. It was difficult to exclude from the apparatus, on admitting the muriatic acid gas into it, two or three very minute globules of mercury, which became tarnished during the experiment, exactly as they would have been by oxymuriatic acid; and a small portion of the latter gas was probably also taken up by the water employed to absorb the muriatic acid.

With the intention of giving greater effect to the electricity, I repeated the experiment in a vessel capable of containing not more than 1400 grains of quicksilver (about ·41 of a cubic inch), the neck of which, being only $\frac{1}{5}$ of an inch in diameter, was better calculated to show any minute change in the volume of the gas. On removing the stop-

* Phil. Trans. 1800, p. 192. — Phil. Mag. vol. vii. p. 214.

† Phil. Trans. 1809.—Phil. Mag. vol. xxxiv.

per, however, no change of volume was apparent. The hydrogen evolved, instead of being more than in the former experiment, equalled in bulk only 20 grains of mercury. The production of oxymuriatic acid was sufficiently evinced by its effect in tarnishing some very small globules of quicksilver, which adhered to the inside of the vessel; but the minuteness of the quantity frustrated an attempt to measure it. From subsequent experiments on similar quantities of gas, confined in the same apparatus, it appeared that the electrization in this last instance had been continued much longer than was necessary; and that an equal effect was produced by $\frac{1}{8}$ the number of electrical discharges.

In this way of making the experiment, the greatest proportion of hydrogen gas obtainable from muriatic acid amounted only to about $\frac{1}{70}$th, while, by electrization over quicksilver, $\frac{1}{16}$ or $\frac{1}{14}$ was generally evolved. It was evident, then, that the mercury had considerable influence over the results; and I found, by experiments with tubes of different diameters, that the larger the surface of the mercury exposed to the gas, the more rapid and complete was the change. Its action was greatly accelerated, also, by causing the electric discharge to strike from the conducting wire, sealed into the tube, to the mercury, which was probably thus raised into vapour; for, in some instances, the whole of the inner surface of the glass was coated with sublimed calomel.

The only way in which the mercury appeared to me likely to be efficient in this case, was by removing the oxymuriatic acid as fast as it was formed; for I have never found any mixture of this gas in the results of experiments on muriatic acid, when carried on over quicksilver. Upon any theory of the constitution of muriatic acid, it may be expected that when, in a mixture of that acid gas with hydrogen and oxymuriatic acid gases, the two latter come to bear a certain proportion to the former, they will be brought within the sphere of mutual agency, and will reproduce muriatic acid. This point appears, from my experiments, to be attained, when the hydrogen and oxymuriatic acid, taken together, have the proportion to the muriatic acid of about 1 to 35. The amount of the change, therefore, which is capable of being effected on muriatic acid gas, electrified without the contact of mercury, is limited by the reaction of the evolved hydrogen and oxymuriatic acid gases on each other, whenever they compose a certain proportion of the mixture. This proportion being

ing attained, we only, by continuing the electrization, work in a circle.

It may now be inquired, what is the limitation to the action of electricity on muriatic acid gas which is confined over mercury? In this case, it was suggested to me by Mr. Dalton, who favoured me with his presence at most of the experiments, that the evolved hydrogen might possibly in some way prevent the effect from being carried beyond a certain amount. Availing myself of this hint, I mixed 30 measures of hydrogen gas with 400 of muriatic acid gas in its ordinary state, and passed 900 discharges through the mixture. It soon became evident that the addition of the hydrogen had produced an important difference in the results of the experiment; for the surface of the mercury, over which the gas rested, was untarnished after some hundred explosions, and was scarcely changed at the close of the process. When the residuary gas, the volume of which remained unaltered, was analysed, it was found to contain the same quantity of muriatic gas as at the outset, and neither more nor less hydrogen. To explain the event of this modification of the experiment, on the old theory, we may suppose that by the action of electricity a particle of water is decomposed, and that the atom of oxygen, forcibly repelled from that of hydrogen with which it was associated, finds another atom of hydrogen uninfluenced by the electric fluid, and within the sphere of its attraction. With this it unites, and recomposes water. On the theory of Sir H. Davy, the same series of decompositions and recombinations may be assumed to take place between the oxymuriatic acid and hydrogen*.

It still, however, remains to be determined, what is the source of the hydrogen gas, which, in a limited proportion, is always evolved by the electrization of muriatic acid? Does it result from the decomposition of water, existing as an element of the gas; or from the disunion of the oxymuriatic acid and hydrogen, which, according to Sir H. Davy's view, compose muriatic acid? The limitation to

* I am aware that there is an apparent inconsistency in supposing changes of precisely an opposite kind to be effected by the same means. But instances are not wanting, in which the very same elements are brought into combination by electric discharges, and are again disunited by the same agency. As examples, it may be sufficient at present to state, that nitrous acid and nitrous gas are generated by the action of the electric spark on mixtures of oxygen and nitrogen gases; and that, by the same power, they are again resolved into their elements. If this were the proper place, it might, I think, be rendered probable by several arguments, that electricity, when thus applied, acts rather by mechanical collision, than by inducing a change in the electrical states of the elements of bodies.

 its

its amount, which, it formerly appeared to me*, could only be accounted for by the complete destruction of the water contained in the gas, may now be equally well explained on the principle which I have just pointed out. The fact, also, that no appreciable change of bulk is produced by the electrization of the muriatic acid, when the presence of mercury is excluded, is perhaps favourable to the new theory. For, since equal measures of hydrogen and oxymuriatic acids afford muriatic acid without any condensation of volume, no alteration of bulk should result from the disunion of those elements; and the products should be equal measures of the same gases. The proportions which I obtained (100 to 140) did not, it must be acknowledged, exactly correspond with the theory; but the difference was not greater than might naturally be expected from the circumstances of the experiment. That equal measures of hydrogen and oxymuriatic acid are really evolved, appears to me to be proved by the agreement, which I have in several experiments remarked, between the hydrogen gas obtained, and the contraction of volume in muriatic acid electrified over mercury. Now the latter effect of the process can be explained on no other principle than the absorption of oxymuriatic acid by the quicksilver.

When muriatic acid and oxygen gases are electrified together over mercury, a gradual diminution ensues in their bulk †, and the mercury becomes tarnished, precisely as by the contact of oxymuriatic acid. I have lately examined the agency of this process on a considerable quantity of the two gases confined in a vessel, into which they were admitted after exhausting it by the air-pump. The phænomena, which in this way of making the experiment are extremely decisive and interesting, are the production of water and of oxymuriatic acid. The former, combining with a portion of the undecomposed muriatic acid, is deposited in drops upon the inner surface of the vessel, in the state of liquid muriatic acid. When the stop-cock, which confines the gases, is opened under mercury, a quantity of that metal rushes in, and has its surface instantly tarnished. Besides this test of the production of oxymuriatic acid, its presence is rendered unequivocal (after absorbing the undecomposed muriatic acid by a few drops of water), both by its smell, and by its effect in discharging the colour of litmus paper ‡. These

* Phil. Trans. 1800, p. 200.—Phil. Mag. vol. vii. p. 217. † Phil. Trans. 1800, p. 193.—Phil. Mag. vol. vii. p. 215.

‡ Those who wish to repeat this experiment need not be deterred by the apprehension of the labour attending it; for 3 or 400 discharges, from a Leyden

These results, it will be found, may be reconciled with either theory. According to the one which has been commonly received, the oxygen unites with the real acid of muriatic gas, which becoming oxymuriatic acid, *deposits* water. On Sir H. Davy's view, the oxygen unites with the hydrogen of the muriatic acid, and *composes* water, while the oxymuriatic acid is merely an educt. I am not aware of any refinement of the process, by which the value of these two explanations can be compared. Something, however, would be gained by a precise determination of the proportions in which the two gases saturate each other. For since, on Sir H. Davy's theory, muriatic acid contains half its volume of hydrogen gas, two measures of which are known to be saturated by one of oxygen, it follows that muriatic acid gas should be changed into oxymuriatic by one-fourth of its bulk of oxygen. According to Gay Lussac and Thenard *, three measures of muriatic acid should condense one of oxygen (or only one-third their bulk), and should form two measures of oxymuriatic acid. Hitherto, I have not been able to satisfy myself respecting the true proportions of oxygen and muriatic acid gases, that are capable of being united by electricity; for, though I have made several experiments with this view, they have not agreed in yielding similar results. The condensation of a part of the undecomposed acid by the water, which is formed during the process, will probably, indeed, always be an impediment to our learning these proportions exactly. The fact is chiefly of value, as it affords an example of the production of oxymuriatic acid under the simplest possible circumstances; and as it shows unequivocally that, under such circumstances, the visible appearance of moisture is a part of the phænomena.

Manchester, Jan. 6, 1812.

LXI. *Letter from Mr.* John Stancliffe *to Mr.* James Allan, *respecting Mr.* Allan's *improved Dividing Engine.*

Sir, The Editor of the Retrospect I find seems still dissatisfied with what I said some time back, in answer to the

a Leyden jar of moderate size, are sufficient to occasion a distinct precipitation of moisture. When a mixture of oxygen and muriatic acid gases is even suffered to stand over mercury, a gradual contraction of volume takes place; the muriatic acid, if in proper proportion, entirely disappears; and calomel is deposited upon the surface of the glass vessel: but, in this case, there is no visible production of moisture.

* *Mémoires d'Arcueil*, ii. 217.

 observa-

observations and erroneous conceptions which appeared in No. 30 of that work, relating to your Dividing Engine. This learned critic I believe is determined, right or wrong, to have the last word; and I am also determined, after a few remarks, to give him no further interruption. I well know that I have no skill in book-making. What I have said on your machine was my first essay, for which I am accused of arrogance, confused and obscure explanation, &c. Now, as that is the case, I may as well add a little more to it, by declaring that I think my judgement in book-making, so far as machinery used in the mathematical line is concerned, is at least equal to that of my accusers,—probably, pretty much upon a par.

In the first place, no other part of your Dividing Engine was offered to the Society of Arts, for their consideration, but a model of that on which the teeth are cut, and that was merely to give some idea of a new plan, for producing a certain number of equal teeth round a circle, with more accuracy than had yet been done. The discussion therefore rested solely on this one point, and was ultimately given in your favour, and for the communication, the Society of Arts have thought proper to honour you with the gold medal.

After all this, the Editor of the Retrospect seems still disposed to cavil, and will not admit of any merit in your invention. The reason I believe is this, that he does not perfectly understand it. He is, probably, ignorant that on your plan there are two rings in use, of the same diameter and thickness, with an equal number of teeth in each, the upper one moveable and the other fixed, perfectly concentric to each other, and also to the centre of the machine from which they move,—and so contrived, that when the two rings are brought into contact and fastened together, they appear as one only. In this state the racking is to commence, and it is to be carried on to about half the depth, on Ramsden's plan, (which I would then abandon). Perhaps there may now exist an error of $\frac{1}{1000}$th part of an inch, which is a great deal. No matter if it was ten times more. Your plan, which now is to take place of cutting and shifting as described in my last, will soon reduce it to nothing, and the operator may cut away, and look out at the window almost all the time, and find that all his work has been still going on right.

Merely saying it is so, because it is so, will not, I find, satisfy every body: absolute geometrical demonstration is the demand. The whole of the operation in this case is purely mechanical, and a very fortunate one it is.

Respecting

Respecting the equality of the teeth of your dividing machine, whatever error there may be before the shifting of the moveable ring, it is clear, the same must exist in them both, being in contact and cut together at the same time. After being reversed and worked round with the cutter a few times, which will produce opposite teeth all the way round, as stated in my last, the next move must be one quarter, or 90°, and this leads me to the grand point at once. I will now suppose that there is some considerable error, however. In this situation there will be found four teeth, and no more, in perfect coincidence and in their true place, on the upper and lower ring, at the four quarters, and by this move it is clear that the largest teeth are placed so as to meet the smallest on the opposite ring. Consequently, when the cutter is turned round, its whole tendency will be to equalize the teeth, which are now under the reciprocal control of each other, so that, by shifting and racking a few times more, this business will be finished to the greatest degree of exactness: the proof of which is simple and easy, it lies in the machine itself, which is, after the cutter is taken away, to move the ring 90°, and then examine the coincidence in every part, which from the nature of the process, I have no doubt, will be found perfectly correct. If ocular demonstration will satisfy, here it may be had.

In all that I have said on this subject, my aim has been not elegance of style, but plainness and precision;—and I think, whoever cannot understand it so far as to enable him to make such a machine, would be unfit to undertake it on other accounts.

I am, sir,

Yours truly,

JOHN STANCLIFFE.

Little Mary-le-Bone Street,
Nov. 19. 1812.

LXII. *On the Motions of the Tendrils of Plants. By* THOMAS ANDREW KNIGHT, *Esq. F.R.S. In a Letter to the Right Hon. Sir* JOSEPH BANKS, *Bart. K.B. P.R.S.**

MY DEAR SIR, THE motions of the tendrils of plants, and the efforts they apparently make to approach and attach themselves to contiguous objects, have been supposed by many naturalists to originate in some degrees of sensation and perception: and though other naturalists have

* From the Philosophical Transactions for 1812, part ii.

have rejected this hypothesis, few or no experiments have been made by them to ascertain with what propriety the various motions of tendrils, of different kinds, can be attributed to peculiarity of organization, and the operation of external causes. I was consequently induced, during the last summer, to employ a considerable portion of time to watch the motions of the tendrils of different species of plants; and I have now the pleasure to address to you an account of the observations I was enabled to make.

The plants selected were the Virginia creeper (the ampelopsis quinquefolia of Michaux), the ivy, and the common vine and pea.

A plant of the ampelopsis, which grew in a garden pot, was removed to a forcing-house in the end of May, and a single shoot from it was made to grow perpendicularly upwards, by being supported in that position by a very slender bar of wood, to which it was bound. The plant was placed in the middle of the house, and was fully exposed to the sun; and every object around it was removed far beyond the reach of its tendrils. Thus circumstanced, its tendrils, as soon as they were nearly full grown, all pointed towards the north, or back wall, which was distant about eight feet: but not meeting with any thing in that direction, to which they could attach themselves, they declined gradually towards the ground, and ultimately attached themselves to the stem beneath, and the slender bar of wood.

A plant of the same species was placed at the east end of the house, near the glass, and was in some measure skreened from the perpendicular light; when its tendrils pointed towards the west, or centre of the house, as those under the preceding circumstances had pointed towards the north and back wall. This plant was removed to the west end of the house, and exposed to the evening sun, beng skreened, as in the preceding case, from the perpendicular light; and its tendrils, within a few hours, changed their direction, and again pointed to the centre of the house, which was partially covered with vines. This plant was then removed to the centre of the house, and fully exposed to the perpendicular light, and to the sun; and a piece of dark-coloured paper was placed upon one side of it, just within the reach of its tendrils; and to this substance they soon appeared to be strongly attracted. The paper was then placed upon the opposite side, under similar circumstances, and there it was soon followed by the tendrils. It was then removed, and a piece of plate glass was

substituted;

substituted; but to this substance the tendrils did not indicate any disposition to approach. The position of the glass was then changed, and care was taken to adjust its surface to the varying position of the sun, so that the light reflected might continue to strike the tendrils; which then receded from the glass, and appeared to be strongly repulsed by it.

The tendrils of the ampelopsis very closely resemble those of the vine, in their internal organization, and in originating from the alburnous substance of the plant; and in being, under certain circumstances, convertible into fruit stalks. The claws or claspers of the ivy, to experiments upon which I shall now proceed, appear to be cortical protrusions only; but to be capable (I have reason to believe) of becoming perfect roots, under favourable circumstances. Experiments in every respect very nearly similar to the preceding, were made upon this plant; but I found it necessary to place the different substances, to which I proposed that the claws should attempt to attach themselves, almost in contact with the stems of the plants. I observed that the claws of this plant evaded the light, just as the tendrils of the ampelopsis had done; and that they sprang only from such parts of the stems as were fully, or partially, shaded.

A seedling plant of the peach tree, and one of the ampelopsis and ivy, were placed nearly in the centre of the house, and under similar circumstances; except that supports, formed of very slender bars of wood, about four inches high, were applied to the ampelopsis and ivy. The peach tree continued to grow nearly perpendicularly, with a slight inclination towards the front and south side of the house, whilst the stems of the ampelopsis and ivy, as soon as they exceeded the height of their supports, inclined many points from the perpendicular line, in the opposite direction.

It appears therefore that not only the tendrils and claws of these creeping dependent plants, but that their stems also, are made to recede from light, and to press against the opake bodies, which nature intended to support and protect them.

M. Decandole, I believe, first observed that the succulent shoots of trees and herbaceous plants, which do not depend upon others for support, are bent towards the point from which they receive light, by the contraction of the cellular substance of their bark, upon that side, and I believe his opinion to be perfectly well founded. The operation of light upon the tendrils and stems of the ampelopsis and

ivy

ivy appears to produce diametrically opposite effects, and to occasion an extension of the cellular bark, wherever that is exposed to its influence; and this circumstance affords, I think, a satisfactory explanation why these plants appear to seek and approach contiguous opake objects, just as they would do, if they were conscious of their own feebleness, and of power in the objects, to which they approach, to afford them support and protection.

The tendril of the vine, as I have already stated, is internally similar to that of the ampelopsis, though its external form, and mode of attaching itself, by twining round any slender body, are very different. Some young plants of this species, which had been raised in pots in the preceding year, and had been headed down to a single bud, were placed in a forcing-house, with the plants I have already mentioned; and the shoots from these were bound to slender bars of wood, and trained perpendicularly upwards. Their tendrils, like those of the ampelopsis, when first emitted, pointed upwards; but they gradually formed an increasing angle with the stems, and ultimately pointed perpendicularly downwards; no object having presented itself to which they could attach themselves.

Other plants of the vine, under similar circumstances, were trained horizontally; when their tendrils gradually descended beneath their stems, with which they ultimately stood very nearly at right angles.

A third set of plants were trained almost perpendicularly downwards, but with an inclination of a few degrees towards the north; and the tendrils of these permanently retained very nearly their first position, relatively to their stems; whence it appears that these organs, like the tendrils of the ampelopsis, and the claws of the ivy, are to a great extent under the control of light.

A few other plants of the same species were trained in each of the preceding methods; but proper objects were placed, in different situations near them, with which their tendrils might come into contact; and I was by these means afforded an opportunity of observing with accuracy the difference between the motions of these and those of the ampelopsis, under similar circumstances. The latter almost immediately receded from light, by whatever means that was made to operate upon them; and they did not subsequently show any disposition to approach the points from which they once receded. The tendrils of the vine, on the contrary, varied their positions in every period of the day, and after, returned again during the night to the situations

situations they had occupied in the preceding morning; and they did not so immediately, or so regularly, bend towards the shade of contiguous objects. But as the tendrils of this plant, like those of the ampelopsis, spring alternately from each side of the stem, and as one point only in three is without a tendril, and as each tendril separates into two divisions, they do not often fail to come into contact with any object within their reach; and the effects of contact upon the tendril are almost immediately visible. It is made to bend towards the body it touches, and, if that body be slender, to attach itself firmly by twining round it, in obedience to causes which I shall endeavour to point out.

The tendril of the vine, in its internal organization, is apparently similar to the young succulent shoot, and leaf-stalk, of the same plant; and it is as abundantly provided with vessels, or passages, for the sap; and I have proved that it is alike capable of feeding a succulent shoot, or a leaf, when grafted upon it. It appears therefore, I conceive, not improbable, that a considerable quantity of the moving fluid of the plant passes through its tendrils; and that there is a close connection between its vascular structure and its motions.

I have proved in the Philosophical Transactions of 1806, that centrifugal force, by operating upon the elongating plumules of germinating seeds, occasions an increased growth and extension upon the external sides of the young stems, and that gravitation produces correspondent effects; probably by occasioning the presence of a larger portion of the fluid organizable matter of the plant upon the one side, than upon the other. The external pressure of any body upon one side of a tendril will probably drive this fluid from one side of the tendril, which will consequently contract, to the opposite side, which will expand; and the tendril will thence be compelled to bend round a slender bar of wood or metal, just as the stems of germinating seeds are made to bend upwards, and to raise the cotyledons out of the ground; and in support of this conclusion I shall observe, that the sides of the tendrils, where in contact with the substance they embraced, were compressed and flattened.

The actions of the tendrils of the pea were so perfectly similar to those of the vine, when they came into contact with any body, that I need not trouble you with the observations I made upon that plant. An increased extension of the cellular substance of the bark upon one side of the tendrils, and a correspondent contraction upon the opposite side,

side, occasioned by the operation of light, or the partial pressure of a body in contact, appeared, in every case which has come under my observation, the obvious cause of the motions of tendrils; and therefore, in conformity with the conclusions I drew in my last memoir, respecting the growth of roots, I shall venture to infer, that they are the result of pure necessity only, uninfluenced by any degrees of sensation, or intellectual powers.

I am, my dear sir,
With much regard, &c.

Downton, April 27, 1812. THO. ANDREW KNIGHT.

LXIII. *On some Combinations of Platina. By* EDMUND DAVY, *Esq., of the Royal Institution. Communicated by the Author.*

[Continued from page 278.]

WHILST that part of my paper in the Philosophical Magazine for November was printing, I obtained some new combinations of platina. My leisure hours since this period have been devoted to the examination of these substances, yet the time has been by no means sufficient to enable me to complete the investigation. I could wish to have made some experiments which were connected with the subject, and to have repeated others on a larger scale. As, however, the facts I have to relate, are for the most part new, no apology, I trust, will be necessary for bringing them forward in what may be considered an imperfect state.

The order in which the details will be made, connected with the examination of these compounds, is of little importance: as an acquaintance with the sulphate of platina must necessarily precede the knowledge of the other combinations, it would seem proper first to describe this substance.

Of Sulphate of Platina.

Sulphate of platina appears to have been first slightly noticed by M. Proust* in a letter to M. Vauquelin; but it has not I believe been hitherto examined.

I have obtained it by the agency of strong nitrous acid on the hydrosulphuret of platina previously washed, and in a moist state. The experiment may be easily made in a

* *Annales de Chimie*, tome xlix.

florence

florence flask. — When the hydrosulphuret of platina is treated with strong nitrous acid, an immediate action takes place, heat is excited, the acid is decomposed, nitrous gas is evolved, and sulphuric acid formed. The spontaneous action, however, is very limited, and soon subsides; heat must now be applied, and the materials boiled to dryness. When the excess of nitrous acid is expelled, a little more should be added, to insure the entire conversion of the hydrosulphuret into sulphate. The utmost heat of a large Argand's lamp may now be applied for a considerable time. When the superabundant acid is driven off, and no more fumes can be perceived, the sulphate of platina alone remains, and exhibits the following properties.

Physical and Chemical Properties.

Its colour is very dark brown, approaching to black. It is obtained in the form of a porous crust or cake, resembling a dry cinder, or the carbonaceous matter resulting from the decomposition of sugar by heat. It is very brittle, and easily pulverized. It has a brilliant resinous lustre, somewhat analogous to crystallized blende, and the powder exhibits a granular appearance.—Its taste is rather acid and metallic, with some degree of causticity.—It slightly affects litmus paper. It is very deliquescent, and, of course, readily soluble in water. When exposed in a dry state to the atmosphere for a few minutes, it becomes quite moist, and after a short time assumes the fluid form.—In this state it appears as a dark brown viscid substance, and a single drop of it colours about a gallon of water. The dry sulphate is soluble in alcohol, and in ether. It is soluble in muriatic acid. When it is boiled to dryness in this acid and heated, it is decompounded, the sulphuric acid is expelled, and the insoluble compound of platina and chlorine remains. It is soluble in nitric acid; but this acid may be successively boiled to dryness on it, without apparently altering its constitution. It is soluble in phosphoric acid.—When the aqueous solution of sulphate of platina is treated with pure potash, soda or ammonia, peculiar triple compounds are formed.—Muriate of alumine, and muriate of barytes, also produce peculiar compounds with sulphate of platina. Of these alkaline and earthy substances, more will be said hereafter.

I have not obtained the sulphate of platina in a perfectly crystallized state. I made some slight attempts of this kind, but have not succeeded in determining the form of the crystals.

The

The aqueous solution of sulphate of platina, as M. Proust has stated, is not precipitated by muriate of ammonia; but if the solution be boiled to dryness, the yellow compound is formed.

The sulphuric acid cannot, it would seem, be separated from the aqueous solution of sulphate of platina, by any of the common methods, even though an excess of nitric acid is present. Acetate and nitrate of lead occasion in it a dark brown coloured precipitate, which I have not examined, but which is probably a triple compound of the oxides of lead and platina with sulphuric acid.

When the dry sulphate of platina is heated to redness in close vessels over mercury, it is entirely decompounded; the platina remains in its metallic state. The gaseous products are oxygene gas, with a little sulphureous acid gas, and a fluid is obtained which evolves copious white fumes when exposed to the atmosphere, and is probably concentrated sulphuric acid, impregnated with sulphureous acid gas;—a fluid precisely analogous to the glacial sulphuric acid of some authors.

Analysis.

To determine the composition of this substance, I decompounded it by the agency of heat, in small retorts over mercury, and over water; and from a comparison of the results furnished by both methods, I shall venture to state the proportions of its constituent parts.

I hoped to have been able to separate all the acid from the sulphate of platina by heat, and thus to have obtained the pure oxide alone; but this was quite impracticable, as both the oxygene and acid appeared to be expelled at the same temperature. In analysing this substance, some precautions were necessary, to avoid the powerful agency of the atmosphere on it.—It could not be pulverized and transferred to any vessel, or weighed, without acquiring an increase of weight.—The dry cake of sulphate of platina was therefore broken into small pieces whilst it was quite hot, and then exposed to the utmost heat of a lamp for about two hours. It was then hastily transferred into a very small ground stoppered bottle previously dried and heated. The bottle was now poised in a very delicate balance, and some of the sulphate put into a small warm retort; the weight the bottle had lost, indicated, of course, the quantity of sulphate which had entered the retort; the minute portion of air could make no ascertainable difference in the results.

Barometer

Barometer 30°. Thermometer 60° Fah.

Experiment 1.—9·3 grains of dry sulphate of platina were heated to redness in a small retort over mercury, and furnished the following results.

		grains.
Metallic platina		6·100
Oxygene gas 2·2 cubical inches =		0·748
Sulphureous acid gas 0·25 cub. in.	0·170	2·452
Sulphuric acid about	2·282	
		9·300

Barometer 30°. Thermometer 60°.

Experiment 2.—18·5 grains of sulphate of platina were decompounded as in the preceding experiment, and afforded

	grains.
Metallic platina	12·15
Oxygene gas 4·42 cub. in. =	1·50
Sulphuric acid about	4·85
	18·50

This experiment was made over distilled water, with a view to ascertain the quantity of sulphuric acid by muriate of barytes; but as the gas was examined, and dust from the charcoal entered the water, much confidence could not be placed in the results.—10·5 grains of sulphate of barytes were obtained, which indicates only 3·57 grains of sulphuric acid. The sulphureous acid gas absorbed by the water would also interfere in calculations of this kind.

Barometer 30°. Thermometer 60°.

Experiment 3.—15·45 grains of sulphate of platina were decomposed over dry mercury, as in the first experiment, and furnished the following products:

	grains.
Metallic platina	10·10
Oxygene gas 4·2 cub. inches .... =	1·42
Sulphureous acid gas 0·35 cub. inch. and sulphuric acid .... about ..	3·93
	15·45

As the sulphureous acid gas obtained in the preceding experiments must be conceived to arise from the decomposition of the sulphuric acid, it may be disregarded in the calculations.—For when the platina and oxygene, in a given weight of the sulphate, are known, the remainder may fairly be presumed to be acid, or acid and water. Now, though my experiments do not necessarily preclude the presence of water in the dry sulphate of platina, yet they seem to show that, if it contain any, the quantity must be extremely small. This idea may derive additional strength from some probable arguments furnished by the agency of heat on it, and from the results of its decomposition in close vessels over dry mercury.—Thus, when the sulphate in its driest state, after being exposed for a considerable time to a heat just below redness, is suffered to remain for a moment in contact with the cool atmosphere; on heating it again, copious vapours are evolved, and these vapours seem to afford a good indication of the presence of moisture. The fuming acid loses this peculiar property by the smallest quantity of water, and the gas obtained holds some of this acid in mechanical suspension. When a bit of paper in its common state of dryness is let up into the gas, a dense white cloud is instantly produced.—It is not difficult to separate water from other metallic sulphates, as those of copper, zinc, iron, &c.; and there seems to be no improbability in the idea that it may be separated from the sulphate of platina, though the relations of the other sulphates to water, seem to be very different from that of the sulphate of platina.

The three preceding experiments were all made with equal care, yet they do not appear to merit the same degree of confidence. I shall venture to deduce the composition of the sulphate from the two first experiments, as they very nearly agree. But independent of this circumstance, their near coincidence with results derived from the analyses of the fixed alkaline sulphates of platina, and with the rule given by Professor Berzelius relative to the constitution of metallic sulphates, are obvious reasons for the choice.

By calculations, these experiments will be found to furnish the following results for 100 grains of dry sulphate of platina.

	Platina.		Oxygene.		Sulphuric acid.
Experiment 1.—	65·29	+	8·04	+	26·37
———— 2.—	65·67	+	8·12	+	26·21

Now taking the mean of these experiments, 100 grains of dry sulphate of platina will be composed of

65·63	platina	or of	oxide of platina	73·70
8·08	oxygene		sulphuric acid	26·30
26·29	sulphuric acid			
100·00				100·00

Now the sulphate of platina may be presumed to contain the

the metal in its lowest state of oxidation—as the black oxide of platina. And the composition of this oxide, as deduced from these experiments is in 100 parts

89·1 platina
10·9 oxygene

100·0

From experiments in the sequel on the potash sulphate of platina, it appears that the black oxide consists of

89·2 platina
10·8 oxygene

100·0

The coincidence between results obtained by such different modes, could not have been anticipated, and seem to afford strong presumptions of their accuracy.

In making the preceding calculations, I have estimated the weight of 100 cubical inches of oxygene gas at 34 grains.

From a careful analysis of some metallic sulphates, &c. Professor Berzelius observed, that there seems to be a certain uniform ratio between the oxygene in the base and in the acid, and that the former is precisely half as much as the latter; and he conceives it probable that this rule may generally obtain in the combinations of sulphur with other combustible bodies. The sulphate of platina seems to conform almost precisely to this rule. According to the experiments of this distinguished chemist, 100 parts of sulphuric acid contain 59·42 of oxygene. Now 100 parts of sulphate of platina appear to contain 73·7 of oxide of platina at the minimum of oxidation, and 26·3 of acid. But the 73·7 of oxide contain 8·08 of oxygene, and the 26 sulphuric acid contain 15·62 of oxygene.—The difference here is extremely small, being only 0·19 per cent.

The sulphate of platina appears to be an interesting combination, and is distinguished from the other metallic sulphates by very decided characters. At present it is but little known; a more intimate acquaintance with it will probably lead to the solution of a very interesting question; namely, whether oil of vitriol can exist in the fluid form without the presence of water.

7. *Of some Triple Salts of Platina.*

The triple compounds obtained by the agency of the volatile or fixed alkalies on solutions of muriate of platina, have been known for a considerable time: they are characterized by containing muriatic acid, or rather I conceive

 chlorine,

chlorine, and have been distinguished by the terms potash muriate of platina, soda muriate of platina, &c.

In making the details connected with the brief examination of the new triple compounds I have procured, I shall adopt a similar method of nomenclature. Thus, the triple compounds obtained by the agency of the alkalies on sulphate of platina, will be denominated potash sulphate of platina, soda sulphate of platina, &c.

If these names are cumbrous and unphilosophical, they cannot be said, like the preceding ones, to convey false ideas, and they are sufficiently significant to answer the purpose of description.

8. *Of Potash Sulphate of Platina.*

This compound is obtained by neutralizing an aqueous solution of sulphate of platina with a solution of pure potash, and boiling the whole for a few minutes; by this means all the platina is precipitated in combination, as a dark brown substance, and the fluid remains colourless.—After being well washed with distilled water, and dried for some hours at a temperature not exceeding 212° Fahrenheit, it exhibits the following properties.

Physical and Chemical Properties.

Its colour is black or very dark brown: it is in small grains, and has somewhat the appearance of gunpowder. It has considerable lustre resembling blende, or pulverized pit coal.—It has a harsh feel. It is tasteless, and insoluble in water. The atmosphere appears to produce no changes on it. It is not affected by boiling nitric acid. Nitro-muriatic acid exerts very little action on it, even at a boiling heat, yet it is readily soluble in boiling muriatic acid. The presence of nitric acid seems entirely to suspend the agency of muriatic acid on it; for, after a part of it is dissolved in muriatic, the addition of nitric acid prevents any further effect. The muriatic solution when boiled to dryness, and heated, furnishes the insoluble compound of platina and chlorine and muriate of potash. The potash sulphate of platina is insoluble in boiling sulphuric, phosphoric and acetic acids. Ammonia produces no effect on it. When it is boiled in a strong solution of pure potash, no apparent change is produced; but when the solution is boiled to dryness and heated, two distinct compounds appear to be formed: one similar to the potash muriate of platina of a yellow colour, the other of an olive colour, analogous to the substance obtained when that body is heated to redness in close vessels.

It

It is insoluble in alcohol and in ether. When it is heated to redness in close vessels, it is entirely decompounded; the products are oxygene gas, metallic platina, and sulphate of potash which remains intimately mixed with the metal. The platina exhibits the characters of the metal in its purest state, and the presence of the salt cannot be perceived by the eye; the agency of water and of muriate of barytes, however, offer easy modes of detecting this substance.

Analysis.

The simplest mode of analysing this substance appeared to me to be by the agency of heat in close vessels. It was first exposed, for two days, to the variable heat of a sand bath, to render it perfectly dry; it was then cautiously heated in a small retort over mercury, with a view to ascertain the quantity of water it contained. The water, however, could only be partially expelled in this way, without at the same time separating oxygene from the oxide. I shall detail the experiments made to determine its composition.

Experiment 1.—23 grains of potash sulphate of platina were cautiously heated in a small glass retort over mercury; a limpid fluid condensed in the neck of the retort, which was tasteless, and did not affect vegetable colours. The substance now weighed 21·6 grains. Its lustre remained unimpaired, and its colour acquired a lighter shade. The loss of 6 per cent. in this instance was merely water; no gas was expelled.

Experiment 2.—10 grains (of the above 21·6 grains) were decompounded at a red heat, in a small resort, over water, and furnished the following products:

	grains.
Metallic platina in grains, with sulphate of potash	8.55
Oxygene gas 2·68 cub. inches ....=	0.91
Water................................	0·54
	10·00

Experiment 3.—10 grains (of the above 21·6 grains) were decomposed as in the preceding experiment, and afforded the following results:

Metallic platina, with sulphate of potash	8·55
Oxygene gas 2·66 cub. inches........=	0·90
Water..........................	0·55
	10·00

 The

The two preceding experiments afford almost precisely the same results; the metallic platina and sulphate of potash, in both instances, weighed the same; and to diminish the average loss, they were mixed together, and boiled in about a pint of distilled water: by this means all the sulphate of potash was dissolved, and the platina alone remained. After the aqueous solution of sulphate of potash had been decanted off, and the grains of metal well washed and heated to redness, they weighed 14·8 grains.

Hence 10 grains of potash sulphate of platina afforded

	grains.		
Metallic platina..	7·40	or 100 grains,	74·0 platina.
Oxygene	0·90		9·0 oxygene.
Sulphate of potash	1·15		11·5 dry sulphate of potash.
Water..........	0·55		5·5 water.

And 100 grains previous to the expulsion of part of its water by heat, contain 78·32 oxide of platina.
10·84 sulphate of potash.
10·84 water.

100·00

9. *Of Soda Sulphate of Platina.*

This substance is procured in a similar manner as the potash sulphate of platina, by treating sulphate of platina with a solution of pure soda, and boiling the materials for a few minutes. The precipitate, after being well washed with pure water, and dried for a considerable time at a temperature not exceeding 212° Fahrenheit, exhibits properties analogous to those of the potash sulphate of platina.

Its colour is black or very dark brown. It is in small hard lumps. It has a shining resinous lustre, similar to that of the potash sulphate of platina, but rather less distinct. It is tasteless, and insoluble in water. The effects of the mineral acids and alkalies on it appear to be very similar to those produced on the potash sulphate of platina. Its general characters so closely resemble those of the potash sulphate of platina, that I have not observed a single property which may be said distinctly to characterize the one from the other.—Like that substance, it is decompounded by the agency of heat, and this mode I adopted to ascertain the proportions of its constituent parts.

Analysis.

Experiment 1.—18·9 grains of soda sulphate of platina, after being dried for some days at a temperature varying from

from 100° to about 212° Fahrenheit, were cautiously heated in a small glass retort, water only was obtained, and the substance now weighed 18·2 grains.

Experiment 2.—9 grains (of the above 18·2 grains) were decompounded at a red heat in a small retort, and furnished the following products:

Metallic platina, with sulphate of soda	7·65
Oxygene gas 2·35 cub. inches = ...	0·80
Water	0·55
	9·00

Experiment 3.—9 grains (of the above 18·2 grains) were decomposed as in the preceding experiment, and afforded the following results:

Metallic platina, with sulphate of soda	7·68
Oxygene gas 2.40 cub. inches .....=	0·81
Water	0·51
	9·00

The metallic platina and sulphate of soda, from the two preceding experiments, amounted together to rather more than 15·3 grains. After being boiled in a quantity of distilled water, well washed, and heated to redness, the platina weighed 14 grains. Hence, taking the mean of these two experiments, 100 grains of soda sulphate of platina appear to consist of

Platina	77·77
Oxygene	8·90
Sulphate of soda	7·33
Water	6·00
	100·00

Or previous to the expulsion of a part of its water by heat, of

Oxide of platina	84·16
Sulphate of soda	7·11
Water	8·73
	100·00

As I suspected the sulphate of soda in the preceding experiments must have been underrated, I made another experiment, using a larger quantity of the substance.

15 grains of soda sulphate of platina, after being drying for several days on a sand bath, were decompounded at a strong red heat by means of a charcoal fire, and afforded the following results:

	grains.
Oxygene gas 3·75 cub. inches =	1·27
Metallic platina	11·10
Sulphate of soda	1·30
Water	1·33
	15·00

Hence, this experiment indicates the component parts of soda sulphate of platina—

Oxide of platina...	82·46
Sulphate of soda..	8·66
Water...........	8·88
	100·00

The difference in these experiments appears to be owing to the circumstance, that a little of the salt was carried into the neck of the retorts during the expulsion of the water, which was not noticed in the preceding experiments.

In the potash sulphate of platina, and soda sulphate of platina, as in the sulphate of platina, the metal, it is presumed, must be at its minimum of oxidation, in the state of black oxide; and there is a very near coincidence as to the composition of this oxide, furnished by the decomposition of all these substances.

10. *Of Ammonia Sulphate of Platina.*

This compound, like the preceding, is made by neutralizing an aqueous solution of sulphate of platina by pure ammonia, and boiling the fluid for a few minutes. After being washed with distilled water, and dried for some hours at a temperature not exceeding 212° Fahrenheit, it exhibits the following properties.

Physical and Chemical Properties.

It is of a lightish brown colour. It is in fine powder or small loosely aggregated lumps. It is tasteless, and insoluble in water. It undergoes no apparent change by exposure to the atmosphere. It dissolves slowly in cold muriatic acid, and readily at a gentle heat. It is soluble in sulphuric acid at a moderate heat. It is insoluble in nitric acid at a boiling heat. When strong nitric acid is boiled to dryness on it, it is decompounded, and a black substance remains, which I am inclined to think is composed of black oxide of platina with nitrate and sulphate of ammonia. When it is boiled in concentrated phosphoric acid, a small quantity of it is dissolved. It is insoluble in acetic acid. It is decompounded when boiled with a strong solution of pure potash

or

or soda, and black oxide of platina remains; but whether the oxide in this instance is quite pure, I have not had an opportunity of determining. It is insoluble in pure ammonia; but, when boiled to dryness with this alkali, fulminating platina appears to be formed, a compound which I shall presently notice.

The ammonia sulphate of platina possesses in some degree the peculiar properties of detonating compounds. When a little of it is exposed to a moderate heat on a slip of platina, a hissing noise is produced, accompanied with slight explosions, and the substance is partially dissipated in dense fumes. Its solution in muriatic acid affords a dense white precipitate with muriate of barytes. I have made no experiments on this compound, so as to determine its composition with accuracy. One experiment, however, may afford an imperfect approximation of this kind.

8 grains of ammonia sulphate of platina were dissolved in muriatic acid; the solution was boiled to dryness in a platina crucible, and decompounded at a red heat. It furnished 5 grains of metallic platina.

This would seem to indicate in 100 parts.

Oxide of platina	70
Sulphate of ammonia and water	30
	100

11. *Of fulminating Platina.*

This compound is obtained, by introducing dry finely pulverized sulphate of platina into pure ammonia, and suffering them to remain together for several hours. It may also be procured by pouring this alkali on the dry sulphate exposed to the atmosphere, but the former method appears to be the best.—After remaining in contact with the ammonia, it is to be washed with distilled water, and dried at a temperature not exceeding the boiling point of water. I have not been able to examine this substance with much attention. The few experiments I have made with it at short intervals of leisure I shall state.

Its colour is light brown. It is procured in small loosely coherent lumps, or in powder. It is tasteless, and insoluble in water. It explodes at the temperature of 490° Fahrenheit, which was ascertained by introducing a thermometer into mercury heated, so as to explode the powder. When about half a grain in fine powder was gradually heated in a platina crucible, it exploded at once with a loud report. When a little of it in small lumps was heated in in a similar manner, several comparatively feeble explosions occurred. In these instances, as also when it was exploded on glass, the materials were all dissipated, there was not the slightest trace of the platina, which in all probability must be reduced in the process. It does not explode by percussion or friction, nor could I fire it by flint and steel. The detonating properties of this compound are far inferior, and differ materially from those belonging to fulminating silver, gold, and mercury; yet they seem sufficiently distinct to entitle it to a place in this class of bodies, and serve to complete the analogy existing between the noble metals.

Fulminating platina is soluble in sulphuric acid at a moderate heat. It dissolves but slowly in boiling muriatic acid. It is slightly soluble in concentrated phosphoric acid, and the solution is of a very deep colour like the sulphate: on the addition of water, a brown precipitate takes place, and the fluid remains turbid and slightly coloured; but after a short time, all the platina is precipitated, and the fluid remains colourless.—I have not examined this substance, but am inclined to think it is a phosphate of platina.

Fulminating platina is slightly soluble in boiling pure nitric acid: the solution affords a precipitate with muriate of barytes; and with all the alkalies, a yellow precipitate. Whether this yellow substance be an oxide or a triple compound of the alkalies oxide and acid, I have not ascertained. When it is boiled to dryness with nitric acid, a black substance remains, which is probably a triple compound. It is insoluble in acetic acid, and when boiled to dryness with this acid it still retains its detonating properties.

I cannot speak with certainty as to the constitution of this compound; but from the foregoing statements I am disposed to believe, it is analogous to the ammonia sulphate of platina, and only differs from this substance, in the proportions of its constituent parts.

12. *Of Alumine Sulphate of Platina.*

This compound is procured, by treating an aqueous solution of sulphate of platina with muriate of alumine, a copious gelatinous precipitate of a brown colour occurs: after being well washed with pure water, and dried for a considerable time at a temperature not exceeding 212° Fahrenheit, it exhibits the following properties.

Physical and Chemical Properties.

Its colour is black, or extremely dark brown. It is in small lumps. It has a shining resinous lustre, resembling that of potash sulphate of platina. It is tasteless, insoluble in water, and unaffected by the atmosphere. When it is heated on a thin a thin slip of platina, it scarcely undergoes any perceptible change, except that its particles assume a more finely divided state from the loss of water. When it is heated to a strong red in close vessels, water only is obtained, and the substance merely acquires a lighter tint of colour. It appears to be insoluble in all the mineral acids at the common temperature of the atmosphere. It is very slightly soluble in boiling sulphuric and muriatic acids, and insoluble in boiling nitric acid. When it is boiled to dryness with strong muriatic acid, it is decomposed, and a yellowish compound is formed, which is partially soluble in water, and affords a copious precipitate with muriate of barytes.

I have not had an opportunity of making experiments on this substance, so as to determine the proportions of its constituent parts. As might be expected, it contains a large proportion of water; for, when dried for several days on a sand bath, it afforded about 27 per cent. of water. I am inclined to think that sulphate of platina will afford a good test for the presence of alumine, when dissolved in muriatic acid: new experiments, however, are wanting to determine this point with precision.

13. *Of Barytes Sulphate of Platina.*

This substance is obtained by the agency of muriate of barytes on a solution of sulphate of platina. When a solution of muriate of barytes is introduced into a strong solution of sulphate of platina, a brown precipitate shortly occurs: after being washed with pure water and dried, it is the substance in question. I have not examined it with much attention. Its colour is light brown. It is in powder, or in small loosely coherent lumps. When heated on a slip of platina, it assumes a dark brown colour. When heated to redness in a small retort over mercury, no gas is evolved, water is the only product, and the substance becomes of a blackish colour. It is insoluble in boiling muriatic or nitric acid. It is dissolved by warm nitro-muriatic acid. It assumes a black colour when put into strong sulphuric acid, and this acid by the assistance of heat dissolves it. It did not appear to be decomposed, when boiled with a strong solution of pure potash, or soda, or carbonate of soda. I have not had an opportunity of analysing this substance.

Observations, &c.

I have made some experiments on the triple compounds of platina, known by the names potash muriate of platina, soda muriate of platina, &c., and it was my intention to have analysed these substances; but a want of leisure has obliged

obliged me to relinquish this design. I shall, however, state a few facts I have observed relative to these compounds.

The potash muriate of platina, when properly prepared, makes, I presume, a good colour for painters. I am not aware that this substance has been proposed as a pigment, or that the fact is generally known; I shall therefore describe the method I have adopted to procure it, together with the properties which seem to characterize and fit it for this purpose.

The purified platina of commerce should be dissolved in nitro-muriatic acid, composed of about three parts of muriatic and one part of nitrous acid, the solution evaporated nearly to dryness, redissolved in water, filtered and treated with a strong solution of nitrate or muriate of potash, till no further precipitate takes place. The precipitate should be washed with a little distilled water, and dried at a moderate heat. As the purified platina of commerce sometimes contains other foreign metals, it is a necessary precaution, first, to treat a little of the acid solution with a few drops of muriate of ammonia. If the precipitate be of a bright yellow colour, the platina is sufficiently pure for the purpose; if, on the contrary, it be of a reddish or muddy yellow, it should all be precipitated by muriate of ammonia, the metal reduced at a red heat in a platina crucible, again redissolved, and treated as at first proposed.

This substance is of a fine yellow colour. It is not apparently affected by the atmosphere. It mixes with the oils without undergoing any sensible change. It is insoluble in the mineral acids at the common temperature. It is not acted on by a strong solution of caustic potash, soda or ammonia. When well dried, it scarcely contains more than 1 per cent. of water. When heated to a dull red in close vessels, it undergoes little permanent change. At a strong red heat it affords chlorine gas, and an olive substance which I conceive is a peculiar compound which has not been examined. The soda muriate of platina, when dried and heated, furnishes analogous results. The olive compounds obtained in these instances consist, I presume, of chlorine, platina, and the alkali.

From Bergman's experiments, Dr. Thomson* states the existence of triple sulphates and nitrates of platina. The sulphate of platina was unknown to this celebrated chemist, and the nitrate is still unknown. These compounds, from the experiments I have made, appear to be merely the alkaline muriates of platina.

* System of Chemistry, vol. iii. p. 135.

Platina

Platina appears to be characterized no less by its valuable properties than by its disposition to form peculiar triple compounds; and there can be little doubt but that a more extensive acquaintance with the combinations of this metal will add considerably to the number of such substances. It would be an interesting inquiry, whether the same laws which seem to govern the formation of binary compounds, extend likewise to thoseof ternary compounds. For an investigation of this kind no metal seems so well adapted as platina; whether we consider the variety of its compounds, or the simplicity of the results furnished by their decomposition.

Some of the preceding combinations seem to agree with the doctrine of definite proportions, and to correspond very nearly with experiments made by Professor Berzelius of Stockholm. But there is not the same coincidence with others. Whilst I cannot but regret the imperfection of this investigation, I indulge a hope, that should the subject hereafter be thought worthy of a more rigorous inquiry, I shall be able by a careful attention to render it more complete.

LXIV. *Case of Femoral Hernia, with some Peculiarities, sucessfully treated by an Operation. By* JOHN TAUNTON, *Surgeon to the City of London Truss Society, the City and Finsbury Dispensaries, and Lecturer on Anatomy and Surgery.*

MRS. M. aged 30, a patieent in the City of London Truss Society, recommended by Mr. Lincoln, Hatton Garden, has had femoral hernia on the right side for about six months; but this gave very little uneasiness, as the tumour was very small, and could at all times be returned by slight pressure.

July 16, 1812, it became painful and irreducible; the pain was referred to the umbilicus, and was preceded by hiccough, sickness, and vomiting. On the 17th she had a stool, but the symptoms continued; the hiccough, sickness, and vomiting were worse.

On the 19th, 3 A.M. I visited her for the first time, found the tumour small, but tense and irreducible. The pain, on attempting to reduce it, was referred to the umbilicus and region of the stomach. Two of the following pills were directed to be taken every second hour:

℞ Hydrargyri submuriatis gr. x.
Extracti colocynthidis ℨ i.
Opii gr. iii.
Olei menthæ piperitæ gtt. vii. Misce.
Fiant pilulæ viginti.

9. P. M.

9 P. M. The pills remained on the stomach; the hiccough and sickness were less. An enema was directed to be given.

20. 5 A. M. Much the same as on the preceding evening. Another box of pills was ordered to be taken as before; the same as the former, excepting the omission of the opium. The tobacco enema was directed to be given, which had the effect of producing a stool, after which she seemed a little relieved; but the hernia remained in the same state.

4 P. M. Restless, skin dry, tongue furred, the hiccough and sickness worse.

9 P. M. The unfavourable symptoms had all increased considerably. On proposing the operation it was agreed to immediately, and performed soon after 10 P. M. The peritoneal sac was very thin, and contained only a few drops of serum; the contents of the sac was an adipose tumour *, having exactly the appearance of condensed omentum. This tumour grew from the coat of the intestine, by a small neck. No part of the canal of the intestine was included in the stricture.

The stricture was dilated in the usual way inwards and downwards towards the symphysis pubis. Immediately on the part being returned she experienced relief, and said she felt much more comfortable. The wound was dressed with lint and adhesive plaster, secured by a bandage; but no medicine was given.

21. 5 A. M. Has had some sleep; the sickness, hiccough, and pain have subsided. There remains only a sense of soreness in the abdominal viscera; the skin is a little dry.

℞ Syrup. papav. ℥ i.
Liquor ammon. acet.
Mist. camph. aa ℥ iii. M.

3 P. M. Has not had any evacuation by the bowels: gave three of the pills which were ordered yesterday, with three table spoonfuls of the following mixture:

℞ Magn. sulph. ℥ ii.
Inf. sennæ ℥ v.
Tinct. gentian. comp. ℥ i. ſs M.

22. Has had two stools; rested well during the night, and appears quite well.

* This is the second case of this kind I have seen: the first occurred in May last, in a patient of the Finsbury Dispensary, aged 70, who did not apply to the charity till a very late period of the disease, and died in 20 hours after the operation. The diseased parts are preserved in my anatomical museum.

25. Has

25. Has been sitting up, and walking about the room, for the last three days, without any inconvenience. The wound has not healed by the first intention, but the granulations look healthy.

Aug. 1. The wound has been dressed every second day, and is nearly healed; her health is quite good, and she does not feel any inconvenience.

Sept. 20. She has taken her accustomed exercise, and nursed a child without pain or inconvenience. Nor is there the least appearance of any protrusion of the hernia. The bandage has been continued to make pressure on the part, as some objections we started to wearing a truss.

JOHN TAUNTON.

LXV. *An easy Method of forming all Kinds of Ovals, commonly known by the Name of Gardener's Ovals*.*

THE oval, as is well known, is an elliptical figure formed by a curved line, which re-enters into itself, and which is composed of several portions of a circle which have various centres.

The execution and demonstration of this figure pre-suppose a degree of knowledge which every body does not possess; but a method has long been known of forming it with exactitude in a practical manner; and an acquaintance with it will be sufficient for artists, gardeners, and other persons who wish to trace this figure: the following is a description of the method in question:

Every oval has two diameters: one long like the line A B (Plate IX.), which is called the great diameter, and the other short, which is called the small diameter, like the line F H, which forms with A B four right angles, since it is perpendicular to it. These two lines are reciprocally intersected at their middle point, whatever length we may suppose these two diameters to have.

It is requisite to trace a curved line, which passes at the different points A F B H, where the great and small diameter terminate.

With this view, from the middle of the great diameter to the point L trace the small arcs to the points G and D, which ought to be equally distant from the point L, and consequently from the points A and B.

Plant at the point of intersection of these two arcs, C and

* *Bib. Phys. Econ. 2d Semestre* 1811, p. 266.

D,

D, two stakes *a b*, well fixed in the ground, in order that they may not shake. Afterwards take with a string the distance from the point D to the point A, or from the point C to B; double this string and knot it firmly, so that, by placing between the two ends at the extremity a tracing stick, it points precisely at the point A, and the stakes C and D are embraced by this string.

Then guide the tracing stick, with the string well stretched but moving freely, from the point A to the point O, and successively to E, F, G, B, H, and finally to A.

The triangles DCO, DCE, DCF, DCG, represent the various movements of the string turning upon the two centres of the oval to the points D and C.

We may always strike an oval, whatever may be the length of the great and small diameter, by taking the difference which is between them, and by sharing out this difference so as that the half of the length which exceeds the great diameter may be subtracted from each extremity of the line AB, or from the greatest diameter.

Let us suppose, for instance, that the line AB of the great diameter is 20 metres long, and that the line FH of the small diameter is twelve metres long, it is evident that the great diameter will exceed the small by eight metres, the half of which is four metres. We must therefore place a stake four metres from the point A, *i.e.* towards C, and another at a similar distance towards D.

It is right to observe, that the oval will be so much the longer, the nearer the stakes approach the points B and A; in the contrary case, the oval will more nearly approach the form of the circle.

LXVI. *Communication from Mr.* J. Allan *respecting his improved Dividing Engine, for which he received the Gold Medal of the Society of Arts, &c.*

To Mr. Tilloch.

Sir, In compliance with a wish expressed by many, I have long had an inclination to communicate, through the medium of your journal, an account of the causes and circumstances that led me to contrive a self-correcting mathematical dividing engine; but the difficulties which individuals unused to literary composition experience in attempts to convey mechanical ideas, and a diffidence in my own powers, operated so powerfully on my mind, that I doubt whether I should yet have ventured, did I not feel myself called upon by a second illiberal attack from an anonymous writer in

an

an obscure publication ;—a writer who, I presume to predict, will not venture to claim in his own name the merit that may be due to his luminous retrospections.

About ten years back I made a dividing engine, nearly 29 inches in diameter, now in the possession of Mr. Cook of Shadwell, in making of which I met with difficulties of which none can form an idea but those who have attempted to make dividing engines;—difficulties which are now almost entirely removed by the new method that I have contrived. In the first place, in making the former engine I turned a rabbet on the edge to receive a ring of plate brass, in which the teeth were cut; but after I had racked it, that is cut the teeth, I was not satisfied. I took it off and put on a second, which experienced the same fate. I then put on a third, and cut it by divisions with a single cutter, in the same way that a clock-wheel is cut, and then applied the screw-cutter to regulate the teeth. I found that by this method I got nearer to truth than I did in the two former rings. In fact, I found that I got it as true as other engines are in general. But though I did a good deal of dividing for the trade, and even gave general satisfaction to my customers, still I conceived that some method or contrivance might be discovered by which the truth of the teeth would be self-evident. I thought that if I could fix a second screw-cutter directly opposite, and communicate exact motion to both at the same time, this would in a great measure answer the purpose; (on this method I had many conversations with Mr. Jarvis, die-sinker, New-street Hill, Shoe-lane;) but after long consideration, and for many reasons that would be too tedious to mention here, I was obliged to give up the idea as impracticable in my opinion. However, I did not despair; I still entertained hopes of devising some method whereby the teeth of a dividing engine could be cut to a greater degree of certainty than hitherto had been done; and also upon a self-evident principle, that would exclude every degree of doubt respecting its accuracy.

At length a new idea presented itself to my mind, which was, to make a second wheel act on the same centre or axis, to afford me an opportunity to shift the one half of the rack or teeth to different parts of the circle, when under the operation of cutting the teeth. The screw-cutter being fixed so as to cut half of the teeth in the one ring, and half in the other, I directly saw that this method would afford me an opportunity of examining the error of the teeth, and also give the cutter an opportunity to correct errors. The more I thought of this method the better I liked it; but I saw, also, that it was a method that would be attended with considerable expense, and which I could but ill afford at the time, having spent so much of my time and earnings in gaining experience by making the former engine. The next idea was the ring; which I saw would be attended with considerably less expense, and would answer the same purpose if well executed: for here only one particular point required to be carefully attended to, viz. to make the rabbet true which was to receive the ring; for it was evident, that if that were turned sufficiently true, the ring would be sure to shift correctly.

I was so well aware of the necessity of strictly attending to this point, that I did not content myself with having the rabbet turned on its own axis in the lathe; but after I had it fitted on its own stand, where it now acts, I fixed a tool with an adjustment to touch the rabbet where the ring is fitted on; and by moving the wheel horizontally round to the said tool, I found that I got it extremely true. This being done, I fitted on the ring, and then found that it would move very correctly to opposite lines or divisions. Which circumstance gave me full confidence that I had obtained my object.

In the description which I gave to the Society of Arts, I made mention of steady pins, which I find does not please some of the critics. I understand that they imagine that the steady pins are made fast in the rings, but which is not the case. The holes that I broached with the stop-broach were through both substances, and which I got true by shifting the ring to halves and quarters as I broached. I then fitted in steel pins, which would push out and in every time that I shifted the ring. I, however, placed more dependence on the opposite lines than on the steady pins; but these were useful in assisting me to shift the ring correctly to the opposite lines or divisions.

That the anonymous author above alluded to does not understand the subject that he writes on, is evident from one circumstance, namely, his comparing my improvement on the dividing engine of Ramsden with Mr. Troughton's method of dividing large circles by hand;—two things which are as different from each other as light is from darkness. He makes his conclusion (after finding all the faults with my method that he can) by saying, that under all the circumstances of the case, we must give the preference to Mr. Troughton's method of dividing; although Mr. Troughton himself, in his description, says that it has nothing to do with Mr. Ramsden's engine. By hazarding such remarks the

the obscure anonymous gentleman does me more good than he is aware of.

The making of dividing engines has hitherto been considered so very difficult an undertaking, that but few have ventured on the labour; and those that have were men of the first abilities as workmen: therefore, if I have made an engine equally correct with the best that has been made, I conceive that I deserve well of my country, because I have found out a method by which any ordinary workman can make a dividing engine equally correct, if not more so than any engine that hitherto has been made, and that with ease.

I cannot be surprised, much less should I be angry, that a person whose profession is writing, should not understand the value of my invention; for I can find men in the same trade with myself who are equally ignorant, but among them none of those who have ever attempted to make a dividing engine. Those who have made the attempt all acknowledge its value in the strongest terms. Among the latter I may mention a most competent judge, Mr. Stancliffe, who was foreman to the late Mr. Ramsden at the time that the engine which obtained the reward from the Board of Longitude was made; who of course had the greatest share in the making of it, and who has since made one for himself. This gentleman was with me on Saturday last; and after examining the engine for some length of time, (it is a second time he has now seen it), he said that it is impossible to say any thing against it; that it is a principle that carries with it self-evidence; that it is a method that never has been equalled before, nor will it ever be excelled, in his opinion; and that it is one of those happy ideas that sometimes takes place in the course of a man's lifetime.

Mr. Berge, who is now in possession of the engine that the Board of Longitude rewarded, and who lived (as I understand) with the late Mr. Ramsden from his youth, has also examined my engine a second time, and declared it to be, in his opinion, the greatest step towards perfection that has been communicated to the public within his knowledge. Mr. Troughton, I understand, also considers the principle to be excellent.

In fact, every one who knows the difficulty of cutting the teeth of a dividing engine correctly, allows it all the merit that I wish; and all agree that an engine racked or cut in the manner that I have done it, that is by shifting the one half of the teeth to different situations, while under the operation of the cutter, and finally producing full sound and complete teeth all round, is a self-evident proof of its superior accuracy. This I have done, after shifting it to opposite quarters and eighths, perhaps not less than 30 times, while under the cutter, and finally the teeth are complete.

The last time that I shifted it was the 10th of last June, which was one-eighth. I then cut a line on the one side, half on the solid wheel and another on the ring, with the dividing point, which is fixed to have no other motion but to and from the centre of the wheel. I then turned the screw 720 times, which is one half of the number of teeth in the wheel. I then cut a similar line with the same fixed point, and then withdrew the 24 bending screws, and shifted the ring half round. Of course the lines now stand in an opposite situation to what they were when cut, yet they are good coincidents.

The anonymous critic, above alluded to, may, whenever he pleases, satisfy himself as to the accuracy of the coincidence of the lines, by a personal inspection of the engine—which will, I think, put an end to all his supposititious cases respecting the shifting of the ring having a tendency to diminish the teeth. He calls me an obscure individual: the charge is certainly true; and had he added, that my obscurity is the cause that my invention has not been more noticed, it would have been equally true. But of this I am confident, that however obscure I may remain, my invention will continue to live as long as mathematical dividing engines continue to be used; and little as he yet knows of the value of my discovery, I have no hesitation in asserting my opinion, that there never was (nor ever will be) a dividing engine cut on the single edge, in the old way, but would, could the teeth be reversed as in my invention, exhibit considerable errors.—Let any person but make the experiment, and he will be compelled to acknowledge the truth of my statement.

Those who have seen circles divided by my engine, and who are competent judges, all approve of their accuracy. The author of the observations which have appeared in Dr. Rees's Cyclopedia states, that he has seen one of my improved reflecting circles well divided. I have received testimonies to the same effect from a Captain M'Lennan, from South America, and from Lieutenant Phipps, of His Majesty's ship Bellerophon; and at this moment I am busy with another reflecting circle, which I hope will be divided soon,

soon, when I will be glad that the anonymous critic, who says I have never given any proof, would come and examine the engine, and what it produces.

I have only one more remark to offer, for the sake of those who may hereafter make dividing engines: it is to correct a mistaken notion, which I find exists among men even in the mathematical line; some of whom imagine, that the way to rack an engine, is to press the cutter to the edge of the wheel, and to go on at once all round, by turning the screw-cutter until they have effected a complete revolution of the wheel; trusting, at the same time, that they have got the wheel exactly of the size that will take in the number of teeth that they want, by which means they conceive there could exist no error. But such a method, I believe, was never attempted, and I believe never will; and could it be done, the wheel would be too little for the screw, when the teeth became full. To such as might be misled by this idea, the following directions may be useful: The wheel, at first, must be kept as large as possible for the screw-cutter to act properly, and by joining different portions of the teeth, which is done from divisions laid down for the purpose as a guide. Hoping that this communication may prove useful,

I am, sir,
Your humble servant,
James Allan.

Blewitt's-buildings,
Nov. 2, 1812.

P. S. Nov. 13.—Since I wrote the above, I have received the subjoined letter from Mr. Bacon, who has been well known, for a long time, in the mechanical world for his ingenuity. He has had a hand in making several of the old engines; and he has also made several straight line engines that act with rack and screw! To his present engine he has added a circular motion, which he has done upon my principle, and which enables him to speak from experience, having produced full and complete teeth.

Mr. Bennett, of Charles-street, Hatton-garden, mathematical instrument maker, whose opinion is also subjoined, is a man of considerable experience in the mathematical line; he was foreman to the late Mr. Ramsden, and considers my invention to be a great improvement: he has favoured me with his opinion, in writing, which I herewith send you.

To Mr. James Allan.

Sir—Agreeably to your request, I give you my opinion of your improvement on dividing engines, which is the same I had the first time I heard it described, that it is very great. In the first place, you can be at a greater certainty that the teeth are all equal, than you could be if it were solid, and therefore more likely to be correct all round; and, secondly, that which was by far the most difficult, is by your method now rendered the easiest part of the labour, namely, the racking. The framing part being correctly made, and the ring well fitted on, and correctly steady-pinned, and the number of teeth marked in by the cutter, (which may be done without that correct dividing which is absolutely necessary in a solid ring,) then by the help of the steady pins, a blind man may finish the racking; and a man that can see may do it without the steady pins. This I assert from practical experience, having made one on your principle, and which any person may see who desires to have ocular demonstration. To sum up the whole, yours is a self-correcting engine.

I am, sir,
Your humble servant,
JAMES BACON.

No. 130, Chancery-lane,
Nov. 11, 1812.

To Mr. Allan.

SIR—I consider your method of racking your dividing engine the best that can possibly be adopted. If carefully executed, it cannot fail to produce a perfect engine; and I believe it is a method that will be followed in future.

I am, sir,
Your most humble servant,
LEONARD BENNETT.

Charles-steet,
Nov. 12, 1812.

LXVII. *An Account of some Experiments on different Combinations of Fluoric Acid.* By JOHN DAVY, *Esq. Communicated by Sir* HUMPHRY DAVY, *Knt. LL.D. Sec. R. S.**

INTRODUCTION.

Two years ago, I engaged, at the request of my brother, Sir H. Davy, in an inquiry respecting the nature of common fluoric acid gas. My principal object was to ascertain whether silex is essential to its constitution, and whether the proportion is constantly the same. This subject, and experiments on the fluoric and fluoboracic acids, occupied me for about six months. Since that time, the work of MM. Gay Lussac and Thenard has appeared, en-

* From Philos. Trans. for 1812, part ii.

titled

titled "Recherches Physico-Chemiques," in the second volume of which is an elaborate dissertation on fluoric acid. These philosophers, I find, have anticipated many of my results, and consequently very much abridged my labour of detail in the following pages. To repeat what is already known would be useless, I shall therefore confine myself to describe what I have observed, which appears to me yet novel, or different from the observations of the French chemists. The order which I shall pursue, will be that which I observed in my experiments. I shall divide what I have to advance into four parts. The first part will relate to the silicated fluoric acid gas, and to the subsilicated fluoric acid; the second to the combinations of these acids, and of pure fluoric acid with ammonia; the third to fluoboracic acid; and the fourth to its ammoniacal salts.

SECT. 1. *On Silicated Fluoric Acid Gas, and Subsilicated Fluoric Acid.*

The facts which have already been published by MM. Gay Lussac and Thenard and others, appear to me to be sufficient to prove that pure fluoric acid has not yet been obtained in the gaseous state, and that silex, or boracic acid, is requisite that it may assume this form. Were more evidences necessary, I could advance many in point. One circumstance only I shall mention, proving that common fluoric acid gas is perfectly saturated with silex. I have preserved this gas, made by heating, in a glass retort, a mixture of fluor spar and sulphuric acid, for several weeks over mercury in a glass receiver uncoated with wax, without observing the slightest erosion to be produced *.

This gas, with great propriety, has lately been called silicated fluoric. Before I proceed to its analysis, I shall notice what method I have found the best for obtaining it. I have for a considerable time, long before MM. Gay Lussac and Thenard's work was published, added to the mixture of fluor spar and sulphuric acid, a quantity of finely pounded glass, and have thus procured the gas with the greatest facility. The advantages of this addition are considerable. The retort is saved, which otherwise, in less than one operation, would be destroyed; and a much larger quantity of gas is procured from the same materials, and with less

* The sides of the receiver indeed became obscure; but this was not from erosion, but from deposition, as appeared from the transparency and polish of the glass being readily restored by slight friction. What the deposition was, I am ignorant of. After several weeks it was so trifling, as to give only a slight degree of opacity to the receiver.

 trouble

trouble and less heat; the action indeed at first is so powerful, that gas begins to come over before the application of heat is made, and a very gentle one only is required to continue its production.

Previous to its analysis, it was necessary to ascertain the specific gravity of the gas. This I have endeavoured to do. The gas, the subject of experiment, was quite pure, being totally condensed by water. A florence flask was exhausted; in this state, weighed by a very delicate balance, it was .... = 1452·2 grains.

Filled with common air ..... = 1452·2 + 10·2.

Again exhausted .... = 1452·2

Filled with silicated fluoric gas = 1452·2 + 36·45

Hence as 10·2 : 31 : : 36·45 : : 110·78.

Thus it appears that 100 cubic inches of silicated fluoric acid gas, at ordinary temperature and pressure, are equal to 110·78 grains.

When silicated fluoric acid gas is condensed by water, it is well known that part only of the silex is deposited. To obtain the whole, in order to ascertain the proportion in the gas, I have employed ammonia in excess. 40 cubic inches of the gas (barom. 30°, therm. 60°) were transferred in portions of 10 cubic inches at a time to a solution of ammonia. The silex precipitated was carefully collected on a filter, and washed till the water that passed through it ceased to be affected by nitrat of lime. It was next dried, and strongly heated in a platina crucible. It weighed 27·2 grains, and was pure silex. Supposing fluoric acid to be the remaining 17·1 grains, which added to 27·2 grains are equivalent to the weight of 40 cubic inches of the gas, it appears that 100 parts by weight of this gas consist of

61·4 silex
38·6 fluoric acid

100·0

That this estimate may be correct, it is evident, that ammonia should have the property of precipitating the whole of the silex of silicated fluoric gas; which I shall not now endeavour to prove, but leave it to be considered in another part of the paper.

There is no improbability attached to the idea, that silicated fluoric acid gas may, from the manner in which it is prepared, contain a proportion of alkali. To discover whether this was the case, a solution of nitrat of lime was added to the ammoniacal solution neutralized by nitric acid, from which the silex in the preceding experiment had

been

been removed. The precipitate of fluat of lime was separated by filtration. The filtered liquid was evaporated to dryness; and the ammoniacal salt heated in a platina crucible till it was entirely dissipated. The residue had the appearance and taste of quicklime. It was dissolved in acetic acid, and the solution yielded sulphat of lime on the addition of sulphat of ammonia. The liquid was evaporated to dryness, and when the residuum had been heated to dull redness, nothing remained but a little white powder, weighing about a grain, and having all the properties of gypsum. Thus it appears that silicated fluoric acid gas contains no alkali.

My next object was to ascertain the composition of common liquid fluoric acid—that acid obtained by the decomposition of silicated fluoric acid gas by water, and which, on account of the separation that occurs of part of the silex, may with greater propriety be called subsilicated fluoric acid. For this purpose, 43·21 cubic inches, barom. 30·4, therm. 50, or 44 cubic inches at common temperature and pressure, were successively added, two cubic inches at a time, to one cubic inch of distilled water in a small jar over mercury. The whole of this, the gas being pure, was readily condensed. The temperature was somewhat raised. The silex precipitated, formed a gelatinous mass of a blueish colour, which had absorbed all the water like a sponge, so that none appeared fluid. This gelatinous mass was carefully transferred to a filter, and washed with distilled water till it was rendered insipid, and incapable of reddening litmus paper. It retained its blueish hue only whilst moist. When dried and ignited, it was in thin lamellæ, and of a snow-white colour, and surprisingly bulky. It weighed 7·33 grains, and was found to be pure silex. Thus it appears that the subsilicated fluoric acid formed by the decomposition of 44 cubic inches of silicated fluoric acid gas contains 7·33 grains of silex less than the gas itself. Consequently independent of water, which no doubt is essential to this acid, 100 parts of it seem to consist of

54·56	silex
45·44	acid
100·00	

I have endeavoured to ascertain what quantity of silicated fluoric acid gas a given quantity of water will condense. In one instance $\frac{18}{100}$ of a cubic inch of distilled water absorbed 51 cubic inches, barom. 30·5, therm. 60°. The gas was added to the water in a jar over mercury, as fast as it was absorbed. The experiment was stopped, when the

gas,

gas, after having remained in contact with the water a whole night, ceased to be diminished. According to this result, the proper correction being made for the additional pressure, water decomposes about 263 times its bulk of silicated fluoric acid gas.

Dr. Priestley observed, that muriatic acid gas reproduced silicated fluoric gas from the crust of silex formed, when the latter is condensed by water*. This experiment I have repeated, and as it appears to show more correctly the quantity of gas water can condense, I shall describe the result. 2·4 cubic inches of muriatic gas were added to a drop of water, that had previously absorbed one cubic inch of silicated fluoric gas, in a jar over mercury. There was an immediate absorption equal to $\frac{2}{10}$ of a cubic inch. The mixture of silex and subsilicated fluoric acid effervesced, and from an apparent solid became fluid, the whole of the silex gradually disappearing. After the first-mentioned absorption, there was no further. The gas produced was silicated, as appeared from the crust it deposited when removed to water, aud the liquid formed was pure muriatic acid; for, decomposed by concentrated sulphuric, it afforded merely muriatic acid gas, without any silicated fluoric. The evident conclusion from the preceding result is, that water condenses equal quantities of the muriatic and silicated fluoric acid gases, and consequently that the first estimate is too low, and instead of 263 times its bulk, it is probably more correct to say that water to be saturated requires at least 365 times its volume. Neither will this estimate appear inconsistent with the former result, when the deposition of silex is considered as an obstacle to the free exposure of the surface of the water to the gas.

Subsilicated fluoric acid is decomposed by ammonia and the fixed alkalies, and by all the earths that I have made trial of. It is also decomposed by the sulphuric acid and the boracic, as well as by the muriatic acid gas.

Of the particular changes which occur when it is acted upon by the alkalies, I defer giving any account at present, as it is my intention to do it in the next section.

To learn the effect of heat on it, a small quantity of strong acid, pure and transparent, was introduced into a retort connected with mercury. A spirit lamp being applied, about three cubic inches of silicated fluoric acid gas were produced. The neck of the retort was lined with silex in a gelatinous state, and much liquid subsilicated

* Vide Priestley on Air, vol. ii. p. 202.

fluoric

fluoric acid, that had distilled over, was condensed in the colder part of the neck, and was absorbed by bibulous paper, previously introduced to prevent the distilled fluid from entering the jar for the reception of the gas. When the whole of the acid in the bulb of the retort had been evaporated, little or no silex remained.

The general result of this experiment is very different from that which Dr. Priestley, who first made it, obtained. Instead of silicated fluoric acid gas, he procured "vitriolic acid air," sulphureous acid gas.

I have tried also the effect of heat on the siliceous crust, formed by the decomposition of silicated fluoric acid gas, by water; but could obtain no sulphureous acid gas, as Dr. Priestley did only a small quantity of silicated fluoric.

The correctness of Dr. Priestley's observations cannot be doubted. I can only account for his results, by supposing that some sulphuric acid in consequence of the high temperature employed in making the gas was volatilized, and mixed with the subsilicated fluoric acid, and that mercury also was present from the acid being prepared over this metal.

These experiments too oppose another statement relative to a method prescribed for making fluoric acid gas free from silex, by merely heating strong subsilicated fluoric acid in a retort, and collecting the gas over mercury. It is asserted, in chemical works of some reputation, that this process is successful. I have never found it so, having always obtained results similar to those above stated. This I suppose is one of the many errors that have secretly crept into repute, and has been believed, because never subjected to the test of experiment.

The action of concentrated sulphuric acid on subsilicated fluoric acid, is similar to that of muriatic acid gas, occasioning a disengagement of silicated fluoric acid gas. Facts which appear to prove, that water is absolutely essential to the existence of this acid.

Boracic acid decomposes it, in a very different way, not from any predominant affinity for the water, but in consequence of a stronger attraction for the fluoric acid itself. Silicated fluoric acid of course is not produced; but liquid fluoboracic acid and the silex is precipitated in a gelatinous state, as when ammonia is employed.

These are the few principal facts I have to notice respecting this acid. Before I conclude, I shall briefly mention a few other circumstances. Applied to the tongue, in its concentrated state, it produces a very painful sensation, like that

that which strong muriatic acid does, and it has a very similar effect on the cuticle. It does not appear to erode glass, for I have kept it in bottles of this substance more than a month without any action being perceptible. Exposed to the air, it slowly and almost completely evaporates, there being only a very trifling siliceous residue; and when gently heated in an open vessel, it is rapidly dissipated in white fumes.

SECT. II. *On the Combinations of Silicated Fluoric Acid Gas, and the Subsilicated Fluoric, and the Fluoric Acids with Ammonia.*

M. Gay Lussac has shown that silicated fluoric acid gas, like carbonic acid gas, condenses twice its volume of the volatile alkali*. The experiment I have several times repeated, and constantly with the same result, no difference appearing when the acid gas was added in great excess to the alkaline, or the alkaline to the acid. This being the case, and knowing the specific gravities of the two gases †, 100 parts by weight of silicated fluat of ammonia seem to consist of

24·5	ammonia
75·5	acid
100·0	

Silicated fluat of ammonia volatilizes unaltered, if heated by a spirit-lamp in the vessel in which it is formed, and provided moisture be entirely excluded.

Like silicated fluoric acid gas itself, this salt is decomposed by water, and a similar precipitation of silex occurs, and in the same proportion. Thus the salt formed by the union of 30 cubic inches of silicated fluoric gas, and 60 of volatile alkali (barom. 30, therm. 60) in a small glass jar over mercury, being carefully collected and introduced into water, afforded five grains of pure silex, weighed after being well washed and heated to redness.

The saline solution, since part of the silex of the silicated fluoric acid gas is separated during its production, appears to be a subsilicated fluat, or a combination of subsilicated fluoric acid and ammonia. Another mode of making it, more directly proves that this is its composition. When ammonia is added to the subsilicated fluoric acid in excess, this salt is formed without any precipitation. From these facts,

* Vide *Mém. d'Arcueil*, tome ii.

† According to Sir H. Davy, 100 cubic inches of ammonia, barom. 30, therm. 60, weigh 18 grains. It is this estimate which I have taken.

facts, it may be concluded, that independent of water, which appears to be essential to its existence, 100 parts of it consist of

	28·34	ammonia
	71·66	acid
	100·00	

Subsilicated fluat of ammonia has a pungent saline taste. It just perceptibly reddens litmus paper. Slowly evaporated, it forms small transparent and brilliant crystals. The largest I could obtain, appeared to be tetrahedral prisms. The solid salt is very soluble in water, but is not deliquescent. When heated it appears to sublime unaltered. It is curious that the solution of this salt, when evaporated by a heat near its boiling point, powerfully erodes the glass or porcelain vessel, and a residuum of silex appears, on the addition of water, to redissolve the salt. This erosion and residue of silex I have seen produced three times following, with the same quantity of salt. I mention the fact, which, I believe, was before observed by Scheele, without attempting an explanation of it. It may perhaps be said, that as the water evaporates, the affinity of the subsilicated fluat for silex increases.

Subsilicated fluat of ammonia is decomposed by the sulphuric acid, and by muriatic acid gas, and also by the fixed alkalies and by ammonia.

Sulphuric acid expels from it, silicated fluoric gas and hydrated fluoric acid fumes.

Muriatic acid gas acts slowly on it, and effects its decomposition apparently through the medium of its water. A little of the crystalline salt was introduced into muriatic acid gas in a jar over mercury. In a short time some silicated gas was produced, as the siliceous deposition, on the addition of water, indicated. Strong muriatic acid was substituted for the acid gas. Now no apparent change took place; for, on evaporating the acid, the residue, decomposed by sulphuric acid, afforded only silicated fluoric acid gas.

The alkalies form by the decomposition of this salt, the same compounds that they do by their action on subsilicated fluoric acid.

Potash expels the ammonia, and produces the silicated fluat and fluat of potash, as MM. Gay Lussac and Thenard have described.

The changes occasioned by soda appeared to me similar; but the gentlemen just mentioned, assert that this alkali precipitates the whole of the silex, and does not form a triple salt with it and part of the acid.

Ammonia

Ammonia seems to me to separate completely the silex, and by uniting with the pure acid to constitute a true fluat. MM. Gay Lussac and Thenard are of a different opinion. They say that the whole of the silex cannot by this method be removed, but only the principal part. Their reason for this belief is, that on repeatedly evaporating the salt after the addition of ammonia and redissolving it, they have each time observed a residue of silex. If they employed metallic evaporating vessels, the results of my experiments do not agree with theirs; for making use of platina for this purpose, and adding an excess of ammonia, I never detected traces of silex on evaporating the filtered fluat. But our results agree, if they employed glass or porcelain vessels, which fluat of ammonia has the property of corroding.

I now proceed to the consideration of fluat of ammonia; but before I describe some of the properties of this fluat which I have observed, I shall briefly mention the means pursued for ascertaining the proportions of its constituent parts.

The composition of subsilicated fluat of ammonia being known, that of the fluat (granting what is already advanced respecting its formation to be correct) may be inferred from the proportion of silex that a given quantity of ammonia will precipitate. 18 cubic inches of ammoniacal gas were condensed by $\frac{1}{4}$ of a cubic inch of distilled water in a small glass tube over clean mercury. This ammoniacal solution was added to a clear filtered solution of subsilicated fluat of ammonia. A precipitate of silex was immediately produced. After several hours standing, this precipitate was collected on a filter, well washed, and dried and heated to redness. It was pure silex, and weighed 1·6 grains. This experiment, like all the preceding, was repeated, and the result confirmed. In both instances there was an excess of subsilicated fluat. The precipitations were made in a platina vessel, and the solutions were neither heated before or after the separation of the silex. Calculating from this result, 100 parts of fluat of ammonia seem to consist of

76·4	ammonia
23·6	fluoric acid
100·0	

Water appears to be a constituent part of this salt.

It may be rendered neutral by means of a gentle heat, which expels the excess of ammonia employed in its formation. In its neutral state, it has a strong saline taste, and it readily deliquesces when exposed to the atmosphere. Like

Like the neutral carbonats, it is decomposed by heat; but there is this difference between them, part of the pure alkali is expelled instead of the acid, and an acid fluat of ammonia is formed. A gentle heat only is required for the purpose, that of boiling water is nearly sufficient. When the heat is much stronger, the salt fuses and passes off in dense fumes of a most peculiar suffocating odour. The effects of these fumes, when inhaled, are very powerful and disagreeable, and even dangerous, I might venture to say, were I to speak from my own experience. In one instance, when I inhaled only a small quantity, they produced in a few minutes a violent cough and catarrh, and apparent accumulation of blood in the neck and head, and symptoms altogether not unlike those the attendants of apoplexy, which continued for about a quarter of an hour, and then slowly diminished, and gradually disappeared without leaving any permanent bad effect. The fluat of ammonia, when heated in a metallic vessel, appears to sublime unaltered. But the result is different when the experiment is made in a glass one. Ammonia is expelled, the glass is corroded, and subsilicated fluat of ammonia is formed and sublimed. Its action on glass is so powerful, that I have successfully employed it instead of fluoric acid itself, for etching on this substance. It has one advantage, that it is more manageable. The solution may be applied by means of a hair pencil or a common pen to the glass, and the erosion will be produced by exposure to a moderate temperature.

The fixed alkalies, and all the earths that I have tried, decompose this salt; they expel the ammonia, and form true fluats with the acid itself. I have examined all the fluats thus formed, and have endeavoured to ascertain the proportions of their constituent parts; but I am not sufficiently satisfied of the accuracy of the results, to venture to give an account of them.

Sect. III. *On Fluoboracic Acid Gas.*

MM. Gay Lussac and Thenard, who first discovered this gas, obtained it by heating strongly, in an iron tube, a mixture of fluor spar and fused boracic acid. I have found that it may be more easily procured, in greater abundance, and at less expense, by gently heating, in a common glass retort, a mixture of finely pounded boracic acid* and fluor

* Common calcined borax answers the same end, but not so well. Its only recommendation to preference is cheapness.

spar

spar with concentrated sulphuric acid. One part by weight of fused boracic acid, two parts of fluor spar, and about 12 of sulphuric acid, appear to be the proportions best adapted for the purpose. This method will require no explanation when it is considered that boracic acid, as has already been observed, precipitates silex from liquid subsilicated fluoric acid. If the heat is gentle, not nearly sufficient to occasion the ebullition of the sulphuric acid, and the proportions just recommended are used, the retort will not be injured, and pure fluoboracic acid gas will be produced in abundance. When the gas ceases to come over, if the heat is raised, more will be evolved, and there will be distilled over at the same time, a viscid fluid, which is a compound of sulphuric acid and fluoboracic acid gas. Now the operation should be stopped, if the object is to obtain merely pure fluoboracic gas, a long continuation of the heat producing some silicated fluoric. Before quitting the subject, it should be observed that the quantity of sulphuric acid employed is of considerable consequence to the success of the experiment. If too much is used, there is a great loss of gas from the property which sulphuric acid has of absorbing fluoboracic acid gas; and if too little is employed, it soon becomes diluted, and loses the power of generating the gas, though it may still decompose the fluor spar. Both extremes, therefore, are to be avoided, and the proportion of acid mentioned above, as far as my experience goes, appears to be the best.

I have endeavoured to ascertain the specific gravity of fluoboracic gas.

The flask exhausted weighed	1400·5 grains.
Filled with common air	1400·5 + 6·2
Again exhausted	1400·5
Filled with pure acid gas ...	1400·5 + 14·7.

Thus it appears that 100 cubic inches of fluoboracic gas are equal to 73·5 grains.

MM. Gay Lussac and Thenard have described the compound of this gas and water, a fuming fluid, in many respects similar to concentrated sulphuric acid. Like this acid, I have observed that it possesses a slight degree of tenacity, so that it has an oily appearance when poured from one vessel to another; and similar in another respect, it possesses the property of charring animal and vegetable substances, and which the French chemists observed belonging to the gas itself. I have found that water condenses more of this than it does of any other known gas, no less than 700 times its volume. The experiment was then

then made, barom. 30·5, therm. 50, $\frac{14}{100}$ of a cubic inch of water were introduced into a tube over mercury, and the gas, in portions of 5 cubic inches at a time, was added until 100 cubic inches had been absorbed, when the water was apparently saturated. This acid was of the sp. grav. 1·77.

The property which sulphuric acid has of absorbing fluoboracic acid gas has already been noticed. I found that $\frac{1}{2}$ cubic inch of sulphuric acid, of the specific gravity 1·85, condensed 25 cubic inches of the gas, or 50 times its volume. The compound acid was strongly fuming, and appeared more tenacious than pure sulphuric acid, yet not nearly so much so as that compound of the two which distils over during the latter part of the operation of making fluoboracic gas.

This latter compound has some peculiarities. It is so tenacious that it flows very slowly. It appears to be far more volatile than pure sulphuric acid. When poured into water, a dense white precipitate is formed, the exact nature of which I have not yet satisfactorily ascertained; but which is not produced by the direct compound of sulphuric acid, and the fluoboracic.

Sect. IV. *On the Combinations of Fluoboracic Acid Gas and Ammoniacal Gas.*

M. Gay Lussac has combined fluoboracic acid gas with ammonia. He states, that equal volumes of the two gases condense each other*. This I have found to be the case, and I have also found that fluoboracic acid gas condenses twice and even three times its volume of the volatile alkali. The compound observed by M. Gay Lussac is solid, white, and opake, like the ammoniacal salts. The combinations I have obtained are liquid, transparent, and colourless, like water, though they are entirely free from this fluid. They were made by the direct union of the two gases. Five cubic inches of ammoniacal gas were added to the same volume of the fluoboracic gas contained in a small jar over dry mercury. There was a complete condensation of both, and the solid salt was the result. Five cubic inches more of ammonia were introduced. The whole was quickly absorbed, and the solid salt was converted into the transparent fluid. Five cubic inches more were added, which too were slowly absorbed, but without any change of form.

The solid salt volatilizes in close vessels unaltered, on the application of a gentle heat.

* Vide *Mém. d'Arcueil*, tome ii.

Both fluid compounds, when heated, are rendered solid, from the expulsion of part of the ammonia. Exposure to the air is attended with the same change, and the same effect is produced by the muriatic and carbonic acid gases.

Knowing the volumes of the acid, and alkaline gases which combine, it is easy to calculate the proportions of each by weight in the respective salts.

100 Parts consist of	Ammonia.	Acid.
The solid compound	19·64	80·32
The first fluid	32·9	67·1
The second fluid	42·4	57·6

These combinations are curious in many points of view. They are the first salts that have been observed liquid, at the common temperature of the atmosphere, without containing water. And they are additional facts in suport of the doctrine of definite proportions, and of the relation of volumes.

LXVIII. *Notices respecting New Books.*

The Philosophical Transactions of the Royal Society for 1812, Part II. has been published since our last Number. The following are its contents:

1. Observations of a second Comet, with Remarks on its Construction. By William Herschel, LL.D. F.R.S.—2. Additional Experiments on the Muriatic and Oxymuriatic Acids. By William Henry, M.D. F.R.S. V.P. of the Lit. and Phil. Society, and Physician to the Infirmary, at Manchester.—3. Of the Attraction of such Solids as are terminated by Planes; and of Solids of greatest Attraction. By Thomas Knight, Esq. Communicated by Sir Humphry Davy, Knt. LL.D. Sec. R.S.—4. Of the Penetration of a Hemisphere by an indefinite Number of equal and similar Cylinders. By Thomas Knight, Esq. Communicated by Sir Humphry Davy, Knt. LL.D. Sec. R.S.—5. On the Motions of the Tendrils of Plants. By Thomas Andrew Knight, Esq. F.R.S. In a Letter to the Right Hon. Sir Joseph Banks, Bart. K.B. P.R.S.—6. Observations on the Measurement of three Degrees of the Meridian conducted in England by Lieut. Col. William Mudge. By Don Joseph Rodriguez. Communicated by Joseph de Mendoza Rios, Esq. F.R.S.—7. An Account of some Experiments on different

ferent Combinations of Fluoric Acid. By John Davy, Esq. Communicated by Sir Humphry Davy, Knt. LL.D. Sec. R.S.—8. On a Periscopic Camera Obscura and Microscope. By William Hyde Wollaston, M.D. Sec. R.S.—9. Further Experiments and Observations on the Influence of the Brain on the Generation of Animal Heat. By B. C. Brodie, Esq. F.R.S. Communicated to the Society for promoting the Knowledge of Animal Chemistry, and by them to the Royal Society.—10. On the different Structures and Situations of the Solvent Glands in the digestive Organs of Birds, according to the Nature of their Food and particular Modes of Life. By Everard Home, Esq. F.R.S.—11. On some Combinations of Phosphorus and Sulphur, and on some other Subjects of Chemical Inquiry. By Sir Humphry Davy, Knt. Sec. R.S.

LXIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

This Society resumed its sittings on the 5th of November, the Right Hon. Sir Joseph Banks in the chair.

A letter from Sir H. Davy to the President was read.

In this letter Sir H. Davy describes the properties of a new and very extraordinary detonating compound.

It may be formed by exposing chlorine, or oxymuriatic gas, to a weak solution of ammonia, of nitrate or of oxalate of ammonia.

It appears in the form of a yellow oil, heavier than water. It appears to be composed of chlorine and nitrogene. It detonates violently by a heat below that of boiling water, and even explodes by a very gentle friction.

It seems to be by far the most powerful explosive substance known. Sir H. Davy, in endeavouring to collect the products from the explosion of a small particle not bigger than a grain of mustard seed, was severely wounded in the eye, and the vessel in which the experiment was made was broken into pieces.

Sir H. Davy stated in his letter, that the first notice of the existence of such a substance was contained in a letter he received from a gentleman at Paris, but the manner of its preparation was not mentioned; and though said to be discovered a year ago, no reference to it is made in any of the French journals.

On the 12th was read an interesting paper by the Astronomer Royal, Mr. Pond, on the summer solstice and the mural quadrant at Greenwich.

On the 19th, a paper on what is called near-sight, and the best remedies for defective vision, by Mr. Ware, was read to the Society.

*** In our last Volume, p. 73, in a Paper read before the Royal Society, Jan. 23, "On the Structure of the Eyes of Man and Birds, &c." it was stated to have been by Mr. Campion.—The paper, we understand, was by Mr. Philip Crampton, F.R.S.

PHILOSOPHICAL SOCIETY OF LONDON.

During this month the attention of the Society has been directed to two lectures on the subject of Light and Heat, by Mr. W. Henley.

As the ancient theories of light and caloric had undergone so much discussion, and as other new ones had lately been adopted and supported by several illustrious philosophers, it became an interesting subject for the investigation of the Society. In the lecture on Light Mr. H. gave a very able analysis of the curious experiments of Mr. Malus, which confirmed his opinion of the polarity of light; and after the most impartial review, he felt perfectly satisfied with the opinions of Sir Humphry Davy on this point.

On the subject of Caloric he entered at much length, and gave an abstract of the different theories, from that of Zeno to those of the present time. He dwelt more particularly on the two theories maintained by the philosophers of the present day, and his arguments were directed principally against the *material* theory;—that which supposes caloric to be a peculiar and subtle fluid, capable of pervading, in a greater or lesser degree, every kind of matter, and of counteracting, by its repulsive power, the attraction of *cohesion*, an opinion adopted almost universally by the chemists. Mr. H. brought forward a number of facts which it could not satisfactorily explain, but which favoured in the most happy manner that hypothesis, which accounts for the phænomena of heated bodies, by supposing the particles of matter to be endowed with a power of repulsion, and that the effects of heat are only attributable to a rapid motion of the minuter particles of matter, rather of a vibrating kind, or of one round their own axis,—an opinion now warmly supported by Sir H. Davy. The theory was supported by the lecturer by a number of well chosen experiments, which, though not able perhaps to carry conviction to the supporters of the material theory, are yet deserving of their attention, as affording a proof of the impossibility of satisfactorily accounting for all the phænomena of heat by one single theory.

Erratum in our last, page 311.—For *Rev. J. Nightingale*, read *W. C. Pettigrew, Esq.* Curator.

KIRWA-

KIRWANIAN SOCIETY OF DUBLIN.

Nov. 18. A Paper "On the Nature and certain Combinations of a newly discovered Vegetable Acid," was read by M. Donovan, Esq.

In examining the combinations of this acid, the author's principal object was to point out the characteristics by which they may be distinguished from those of other acids. Distinctions so obvious as not to require particular notice, exist between this and all other acids except the malic. Even the latter, although having a resemblance in some instances, is distinguishable by several striking properties. The following abstract, notwithstanding that it does not comprise every particular difference, will sufficiently illustrate the comparative properties of the two acids.

Malic acid is always brown, even when prepared according to the process of Vauquelin.	The new acid is colourless at any degree of concentration.
Malic acid unites to oxide of lead, and forms a powder which cannot by any known means be made to assume the crystalline form.	The new acid unites to lead, and easily forms crystals of a peculiar lustre and beauty.
Malic acid, when presented to carbonate of lime, always forms an acidulous salt, which cannot be rendered neutral even by boiling the malate to dryness, on an excess of the carbonate. The acidulous solution in *some days* deposits crystals.	The new acid, when merely agitated with carbonate of lime, forms a neutral salt, which almost *immediately* precipitates.
The same observations hold good with regard to barytes.	The new acid, when heated on carbonate of barytes, dissolves an *excess of base;* the other properties of the compound are like those of the preceding.
Malic acid forms with magnesia a salt which deliquesces, and which does not assume the crystalline form.	The new acid forms with magnesia a crystallized salt, which requires 28 parts of water at 60°, for solution.

The new acid *decomposes* malate of lead.

GEOLOGICAL SOCIETY.

This society held its first meeting of the present Session on Friday, Nov. 6, 1812.—The President in the Chair.

Sundry presents of books and specimens were reported.

A second letter from Ed. L. Irton, Esq. in answer to some queries by the President, relative to the sand tubes found at Drigg in Cumberland, was read, and thanks were voted for the same.

From this it appears that the tubes have hitherto been found only in a single hill of drift sand on the sea shore, of the extent of about five acres. The entire form of the tubes is not known, for they are discovered in consequence of being laid bare by the drifting of the sand; and the same cause almost always breaks off and injures their upper extremity. The manner in which they terminate below is still less known; one of the tubes was exposed by hazardous digging in running sand, to the depth of about fifteen feet, without the least appearance of its being about to terminate. They lie parallel to each other, and nearly vertical, but at unequal distances; the number must be very considerable, Mr. Irton having himself taken away, at different times, not less than a hundred. The tubes, when first dug out, are very flexible, but exposure to the air for a few seconds deprives them of this quality. The unctuosity of the internal glazing of these tubes, when recently dug up, stated by Mr. Irton in his first letter, on the authority of another person, appears on more accurate examination to be a mistake.

A communication from George Cumberland, Esq. relative to some limestone strata in the neighbourhood of Bristol, was read, and thanks were voted for the same.

The strata here described compose the rocks opposite to the Hotwell walks, and are further illustrated by two drawings; the one of the external face of the rocks, the other of a large cavern recently discovered. In clearing the ground for the erection of houses opposite the old York hotel, on Clifton downs, some interesting varieties of sulphate of strontian were met with; but the place being now covered with building and garden-grounds, there is little likelihood of its being soon again opened to the researches of the mineralogist.

A communication, accompanied by three drawings in illustration from Dr. Mac Culloch, Mem. G. S. relative to a remarkable interrupted vein in limestone, was read, and thanks were voted for the same. This vein occurs in a millstone which was shipped from Limerick, and is at at present at the royal powder-mills at Waltham Abbey. The stone itself is a dark blue slaty limestone, containing comminuted fragments of marine remains; the vein by which it is traversed is whitish compact carbonate of lime. This vein, in its present state, consists of a number of separate angular fragments, having somewhat of a general parallelism, with such a correspondence at any two neighbouring extremities, as to render it a matter past doubt that they have once formed a continuous vein. To displace such a vein into its present position, it is necessary to suppose that the rock originally consisted of a series of very thin strata, which being fissured across, formed a space for the reception of the substance of the vein. It is evident from the angularity, and the irregularly serrated edges of the displaced fragments, that the white calcareous carbonate must have been perfectly indurated at the time of its displacement; yet, that the strata of the limestone must have been in a state to admit of a series of shifts or slides, each successively advancing with equal intervals beyond the one preceding it: it is necessary also to suppose that the strata must have been in some condition admitting them to adhere intimately together, either at the period when the slides took place, or afterwards, from the perfect obliteration of the seam. By what theory can these facts be explained?

Friday, Nov. 20.—The President in the Chair.

Mr. Dunston, having signed the obligation, was admitted a member of the Society.

The Right Hon. —— Dundas was elected an ordinary member of the Society.

Thomas Murdoch, F.R.S. of New Cavendish-street, Portland Place;

George Tuthill, M.D. F.R.S. of Soho Square;

Mr. William Nicholson, civil engineer, Bloomsbury Square;

Thomas Hare, Esq. surgeon, Argyle-street;

were severally proposed as ordinary members of the Society.

The Secretary reported that the following communication had been received, viz. "Observations on a bed of greenstone near Walsall in Staffordshire," by Arthur Aikin, Esq. Secretary.

The Secretary further reported, that the following presents had been received; and thanks were voted for the same, viz.

1. Specimens of rocks from Sicily from Dr. Franck, M.G.S.

2. Specimens illustrative of Mr. Aikin's paper.

3. Two tons of coals for the use of the Society: one from Wyken, near Coventry: the other from Petsall in Staffordshire, from William James, Esq. M. G. S.

A communication from Arthur Aikin, Esq. Secretary, entitled, "Some observations on a bed of greenstone near Walsall, Staffordshire," was read, and thanks were voted for the same.

The greenstone, which is the subject of this paper, is of a dark blackish-blue green colour; has a glimmering lustre, and an uneven fracture, breaking into irregularly wedge-shaped blunt-edged fragments: it is tough, acquiring a kind of polish under the hammer, is moderately hard, and rather heavy. It strongly attracts the magnetic needle, and effervesces on immersion in cold diluted muriatic acid. It consists principally of felspar, mixed with calcareous spar, with minute shining black grains of angite, and of hornblende. It is penetrated by nearly vertical contemporaneous slender veins of calcareous spar, and after a few weeks exposure to the air acquires a liver-brown colour, and falls to pieces.

It occurs in the Independent Coal Formation, but is not co extensive with that formation; nor, indeed, in the opinion of the author of the paper, is it to be considered as a true bed, but rather a lateral vein branching off from a large dyke of greenstone that comes up to the surface, dividing the colliery in which the greenstone bed is, from another adjacent to it.

On comparing the strata above and below the greenstone with the very same strata that have been pierced through in a part of the colliery where the greenstone does not occur, it appears, That the bed of slaty clay, with balls of ironstone lying *upon* the greenstone, does not materially differ from the same bed where the greenstone is absent; but that the beds immediately *below* the greenstone, present very different characters where they are covered by this latter, from what they do where the contrary is the case. These beds are 1. Sandstone; 2. Bituminous shale with slender seams of coal; and 3. A coal somewhat more than a yard thick. Of these, the sandstone is considerably indurated: the bituminous shale is also indurated, entirely deprived of bitumen, and is broken more or less into angular pieces, and mixed with the lower part of the sandstone bed. The yard coal is also entirely deprived of bitumen, is stained and iridescent on the surface of its natural joints, and is more friable. These changes appear to accompany the superposition of the greenstone bed through its whole extent;

extent; and, from the circumstance of their ceasing where the greenstone terminates, they appear to be occasioned in some way or other by this bed.

IMPERIAL INSTITUTE OF FRANCE.

[Continued from p. 314.]

Optics of Ptolemy.

It was thought that this work was entirely lost: a few lines only of it, handed down by Bacon and afterwards copied by Montucla, were supposed to be all that remained.

Count Laplace was the first to announce that the Imperial Library possessed the Latin translation made by Ammiratus Eugenius Siculus. M. Humboldt having read this translation, communicated it to M. Delambre, who made an extract from it too copious to be inserted here at length, and which he read to the Class on the 7th of October. This work, the first book of which was wanting in the Arabic MS. from which Ammiratus translated, contains abundance of obscure metaphysics, physical explanations which are little better, an erroneous system on Vision, which we find more amply detailed in the Optics of Euclid, and in Cleomedes, some true theorems, but demonstrated in a long and tedious manner. A more careful translation would add absolutely nothing to our present stock of knowledge. But the errors even of Ptolemy are not without interest to the history of science: an extract from a few of its pages will satisfy our curiosity in this respect: but notwithstanding these imperfections, the work in question contains two very remarkable articles.

In the first, Ptolemy gives a very precise and complete account of the effects of astronomical refraction. He positively asserts that these effects are the more considerable the nearer the star is to the horizon; that the refraction constantly brings the star to the zenith; that it diminishes in appearance the parallel of the described star, because it generally diminishes its polar distance, except however when the star passes the meridian between the zenith and the pole, because then by drawing the star to the zenith it removes it from the pole: thus, its parallel in this case ought to increase; but then the effect is insensible, because the star is too close to the zenith. In this respect, therefore, Ptolemy was further advanced than Tycho Brahe, Kepler, Hevelius, and all the astronomers to the time of Cassini, who was the first among the moderns to ascertain that

that refraction did not cease entirely but at the zenith. To conclude: Ptolemy contents himself with vaguely pointing out in what way we may determine the quantity of refraction; but he gives no part of it. We were hitherto far from supposing that Ptolemy entertained so expansive and accurate ideas, more especially as there is not a single passage in the whole Almagistus which speaks of refraction, and that it was by a false interpretation that in two places a vague idea was supposed to have been conveyed of their principal effects.

But it is still more curious, that Ptolemy knew as well as the moderns the refraction which the light undergoes in passing from the air into water or into glass, and gives tables for all the angles of incidence fron 10 to 100°. He even points out the method of constructing these tables from observation; and he sees that the angles broken off do not decrease in the same ratio with the angles of incidence. He had no idea of comparing the sines or the chords of the double arcs; but from these tables we may easily deduce the relation of the sines; and what proves that these observations were accurately made, is, that M. Delambre has determined that they differ very little from those which were found by Newton.

The calculations of Ptolemy are 4 : 3,05656 and 3 : 2,0098; those of Newton are 4 : 299432 and 3 : 1,93408; this small difference may arise from Newton's using rain water in his experiments; whereas Ptolemy may have used common water; and perhaps also the common glass used by Newton had not the same density with the purest glass of the ancients.

[To be continued.]

LXX. *Intelligence and Miscellaneous Articles.*

TO THE LOVERS OF NATURAL HISTORY, AND THE GENEROUS AND HUMANE IN GENERAL.

In a Note at page 82 of the 39th volume of the Philosophical Magazine, the Editor took the liberty of requesting the attention of the affluent among his readers, to the distressed situation of the ailing widow with six orphan children, and the aged mother, of the late very ingenious Mr. William Martin, F.L.S. Geol. Soc. of London, &c. of Macclesfield in Cheshire, author of "*Outlines of the Method of Extraneous Fossils*" (an elementary and general work on this interesting subject); of "*Petrificata Derbiensia*, or, Figures and Descriptions of Petrifactions collected ed in Derbyshire," (the result of much local research and patient investigation,) and of a paper on Rottenstone, in the Manchester Memoirs, &c.; and he is sorry to learn by a recent letter from Mrs. Martin, that the generous contributions of her late husband's friends, (for which she desires to express the utmost gratitude,) and the small income that she has from her situation as Librarian to the Macclesfield Reading Society, have proved rather inadequate to the support of her family, and necessitate her to another appeal to the generosity of the public, for enabling her to meet the increasing difficulties of her situation.

The Editor understands that some copies of the two first mentioned works remain yet on sale for the benefit of Mrs. M. in the hands of Messrs. White and Cochrane in Fleet-Street, and who likewise receive and remit the contributions of the feeling and humane; as does also Mr. Wilson Lowry, of No. 57, Great Titchfield-Street; and the Editor would gladly do the same by any sums that may be sent to his Office in Pickett-Place, Temple-Bar.

Mr. W. Davis has in the press a fifth edition of his Treatise on Land Surveying by the chain, cross and off-set staffs; a description of the new invented plan and map meters, and a supplement on conducting subterraneous surveys, greatly improved, enlarged, and newly arranged; to which will be added a portrait of the author.

Also No. XVI. of the Gentleman's Mathematical Companion for 1813, is in great forwardness.

THE FINE ARTS.

The lovers of Grecian sculpture must be pleased to hear, that this country will receive a great and important accession to its stores, in a frize of alto relievo, 100 feet long, found by Messrs. Leigh, Cockerell, Foster, Baron Haller, and Monsieur Linkt, at the Temple of Apollo Epicurius, Phigalia, evidently one of the works of Phidias. From the unconnected state in which the different parts were found, the gentlemen who have examined it have not yet been able to unite it sufficiently well to form an idea of the subject; but, from every opinion formed at present, it appears to contain the two subjects of the quarrel which arose at the marriage of Pirithöus with Hippodamia, and the battle between the Amazons and the Athenians. Of the former there can be no doubt, since many of the events which there occurred are too remarkable to be mistaken, particularly

larly where the centaur Eurythion endeavours to carry away Hippodamia, and is prevented by Theseus; also the two centaurs, who are seen forcibly carrying away the virgin, and the youth her lover. The latter subject is more difficult of divination; and the suggestion of its being the abovementioned, arises only from the persons of Antiope and Theseus being very conspicuous in the groupe. The whole formed the frize of the interior of the Cella, which was of the Ionic order, and the relievo is even higher than that of the temple of Theseus. The opinion of its being the work of Phidias, independently of the style of sculpture, is also strengthened by the circumstance of the temple being built by Ictinus, who generally gave the preference to the above sculptor.

The Members of the Turin Academy have offered prizes of 600 francs for each of the best memoirs on the following subjects:

1. "An account of the origin, appearance or formation of aërolites, either ancient or modern, but founded upon rigorous principles, and upon conclusions from incontrovertible facts, and which will consequently agree with the various atmospherical phænomena which precede, accompany, and follow the fall of an aërolite."

2. "Determine the epoch of the perihelion return of the comet of 1759, generally known by the name of Haller's Comet, taking a minute account of the perturbations."

The Academy requires the reduction into numbers of the analytical forms.

The memoirs are to be transmitted to Turin on or before the 1st August 1813.

THE NEW COMET.

Extract from a Paper read to the French Institute, Aug. 31, 1812. by M. Nicolet.

"A new comet was discovered on the 20th of July, by M. Pons, at Marseilles, and on the 1st of August following by M. Bouvard at Paris, M. Bouvard and myself calculated that the comet would come nearest in contact with the sun on the 15th of September, 92′. 27″. mean time, reckoned from the midnight of Paris.

"The distance from the earth to the sun being taken as unity, that of the comet in its perihelion will be 0,77,835.

The

"The longitude of its ascending node is	253°.	18′.	50″.
"That of the perihelion on the orbit .	92°.	58′.	30″.
"Its inclination on the ecliptic . . .	74°.	20′.	30″.

"The motion of the comet is direct: in addition to its slow motion, it affords a remarkable uniformity in longitude and latitude; and these two circumstances have rendered the calculations more difficult of execution.

"It approaches the earth very slowly. There are some days when we know its place in the heavens, and when the absence of the moon admits of our observing it, on which it may be seen with the naked eye: its tail is nearly two degrees long. It may appear striking to those who shall be able to choose a convenient time and place for observing it; but whatever may be the favourable circumstances under which it presents itself to our vision in France, it is far from being so luminous as the comet of last year. But this is of little consequence to astronomers, who do not found their observations upon the fugitive characters which attend these phænomena. The comet in question was at first seen and calculated upon without a tail, and might have disappeared in this state without causing the least regret among astronomers. If they now pursue its progress until it disappears, it will only be to perfect its elements, and to ascertain if their series of observations furnishes any index on the subject of its revolution."

A private letter from the environs of Mount Caucasus conveys the following intelligence:—"It has been long known that the rich inhabitants and invalids of Russia used to frequent the hot springs of Caucasus, and that they were in great repute. They suddenly disappeared in the month of March last, and re-appeared in April upon the ridge of the mountain, but in much greater quantity, and much hotter than before; but not a drop was to be found in the site occupied by the former buildings. The springs disappeared without any earthquake or other perceptible phænomenon. New bath-houses have been hastily constructed where the springs now flow."

Mr. Bakewell will commence a Course of Lectures on Geology and Mineralogy, at the Surry Institution, in January 1813.

ELECTRICITY.

Mr. Singer will employ his powerful apparatus during the ensuing season, in a Series of Lectures on Electrical and Electro-

Electro-Chemical Science, at the Scientific Institution, Princes Street, Cavendish-Square.

The lectures will embrace every important feature of these interesting subjects, including the latest researches of the continental philosophers, and a variety of original matter. We are informed the lectures will commence toward the close of December.

LIST OF PATENTS FOR NEW INVENTIONS.

To George Paxon, of Hampstead, in the county of Middlesex, upholder, for his improvements in the manufacture of a bedstead or bed-frame to relieve the bed-ridden, the ruptured, and sufferers with broken limbs, gout, or any other affliction.—28th August, 1812.

To Leger Didot, of Prospect Place, Edgware Road, in the county of Middlesex, gent., for his improvement in moulds for making paper.—25th Sept.

To Durs Eegg, of the Strand, in the county of Middlesex, gun-maker, for his improvements in the construction of fire arms and their locks, and also in the apparatus for trying and loading them.—25th Sept.

To Thomas Handford, of the Strand, in the city and liberty of Westminster, cabinet-maker and portable desk-manufacturer, for his travelling trunk on an entire new construction, which will preserve the property deposited therein in a far greater degree than any heretofore made or now in use.—25th Sept.

To John Bunn, of Lower Halliford, in the county of Middlesex, iron hoop manufacturer, for his improved method of manufacturing of rods and hoops from old iron hoops.—25th Sept.

To John Baptish Serny, of King's Road, Chelsea, in the county of Middlesex, doctor of medicine, for his certain improvements in the methods already known and practised of raising sunken vessels or other matters, and in the machinery used for such purposes.—25th Sept.

To Francis Deakin, of Deritend Mills, in the county of Warwick, wire drawer, for his new method of making knife, scissars, and various other cases or sheaths.—23d of October.

To Thomas Pardoe, of Newgate Street, in the city of London, carpet manufacturer, for his new method of working or making carpeting, denominated Kidderminster or Scotch carpeting, in pieces of different widths exceeding 18 inches wide, whereby a complete pattern, figure or flower is made to extend the whole width of the piece, and may

may be worked or made as a drop pattern, or otherwise.—23d Oct.

To John Lewis, of Half-Moon-Street, Piccadilly, in the county of Middlesex, for his improvements on horse shoes and in shoeing horses.—31st October.

To Colonel William Congreve, of Cecil-Street, in the county of Middlesex, for an improved system of securing buildings, towns, dock-yards, and ships from fire, combining a power for the raising of water to the tops of buildings, and for other general purposes.—31st October.

To Edward Charles Howard, of Westbourn Green, in the county of Middlesex, for a process for preparing and refining sugars.—31st October.

To Peter Nouaille, of Greatness, near Seven Oaks, in the county of Kent, Esq. for a method of saving water in mechanical and hydraulic purposes.—31st October.

To Benjamin Cook, of Birmingham, in the county of Warwick, gilt-toy-manufacturer, for an improved method of making or constructing window-blinds, fire-screens, chimney-pieces, sashes, doors, picture-frames, and frames for dressing, pier and other glasses, and various other useful and ornamental articles.—31st October.

To William Caslon the younger, of Dorset-Street, Salisbury-Square, in the City of London, letter-founder, for his improved printing type.—31st October.

To Joseph Bramah, of Pimlico, in the county of Middlesex, engineer, for certain improvements in the method of constructing, laying down, and organizing the main and other pipes for the conveyance of water for the supply of the metropolis of London, and other cities, towns, and places where public water-works are adopted; and applying the water so conveyed to a variety of other useful purposes.—31st October.

To Robert Salmon, of Woburn, in the county of Bedford, surveyor, for improved guards and shades for windows, whereby security is obtained, and the sun and weather more certainly kept off, than has hitherto been effected by any other shade or blind.—31st October.

To William Evetts Sheffield, of Somers Town, in the county of Middlesex, gentleman, for an improved apparatus and furnaces for separating metallic and other substances from their ores, or whatever matters may be combined, united, or mixed with them, and in the application of the same.—31st October.

To Thomas Lea, of Kidderminster, in the county of Worcester, carpet-manufacturer, for certain improvements in the making of carpets.—31st October.

METEO-

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND,

For November 1812.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Oct. 27	45	50°	42°	29·70	0	Stormy
28	40	52	36	·40	10	Stormy
29	34	46	40	·72	26	Fair
30	44	47	42	·62	20	Fair
31	44	48	48	·90	32	Fair
Nov. 1	50	56	46	·90	25	Cloudy
2	50	54	47	·99	20	Fair
3	47	51	40	30·07	22	Fair
4	43	50	40	29·96	27	Fair
5	40	47	38	·84	26	Fair
6	34	46	35	·80	28	Fair
7	30	42	32	·80	20	Fair
8	27	42	37	·79	10	Fair
9	38	44	38	·78	11	Fair
10	32	42	40	30·16	22	Fair
11	40	46	40	29·92	20	Cloudy
12	41	42	45	·72	0	Rain
13	46	54	52	·32	0	Rain
14	50	54	40	·35	6	Cloudy
15	40	47	42	·60	10	Fair
16	45	46	40	·20	0	Rain
17	46	46	44	28·96	0	Rain
18	46	45	38	29·20	0	Rain
19	34	42	33	·70	15	Fair
20	29	40	30	·79	10	Fair
21	30	38	32	30.00	15	Fair
22	32	37	29	·28	10	Fair
23	26	40	40	·27	11	Fair
24	40	45	40	29·92	10	Fair
25	42	46	38	·78	9	Fair
26	40	47	40	·78	5	Foggy

N.B. The Barometer's height is taken at one o'clock.

LXXI. *A Description of a new Instrument for facilitating the Navigation of Ships at Sea, called "The Marine Transit." By* W. Chavasse, *Lieutenant in the 6th Regiment of the Madras Native Infantry.*

To Mr. Tilloch.

Dear Sir, If you deem the following Description of the Marine Transit worthy of insertion in The Philosophical Magazine, it is much at your service.

I remain, dear sir,

Yours very truly,

12, Great Castle Street, Cavendish Square, Dec. 10, 1812. Wm. Chavasse.

The Marine Transit.

The Marine Transit is intended for the same purposes at sea, as the Transit Telescope on land, to afford the means of obtaining a daily correction or adjustment of our time-keepers, whereby the longitude, or a ship's position, with respect to a particular meridian, may be ascertained, and many useful problems relating to astronomy satisfactorily solved. Should this instrument answer the end proposed, the public will be indebted to the Right Honourable the Lords Commissioners of the Admiralty, and the Honourable the Court of Directors of the East India Company, for their fostering care in extending to me the means and patronage necessary to insure the trial of it under the most favourable circumstances.

At the beginning of the present year, an outline of the plan was submitted to the Right Honourable the Lords of the Admiralty, when they were pleased to order that two instruments should be made under my superintendence. I have now the pleasure to announce that the same have been completed by that most competent workman, Mr. Thomas Jones, of Oxendon-street, London; and that we are employed adjusting their rates of going, previous to such a course of experiments on shore, as will determine the propriety of taking them to sea.

The principle on which the instrument is constructed is simple, and promises, if there be no improper assumption in the premises, or mistake in the reasoning, to be efficacious. The principle in few words is this: that equal quantities of mercury will pass through a given aperture in equal times, if the same height of column, reckoned from the discharging aperture, can be uniformly maintained, provided that changes of temperature have no effect on the result; or, if they have, that the quantity of the deviation may be estimated, and added or subtracted as the case may require.

A drawing, with a brief description, will best explain the construction of the instrument, and the way in which it is proposed to apply to practice the principle that has just been stated. (See Plate X.)

The figure is a perspective representation of the Marine Transit, No. 1, complete, and pendent from the deck of the ship. The iron magazine *AA*, containing a sufficiently abundant but an indefinite quantity of pure mercury, is arranged, like the steering compass, to swing in gimbals, so that the parts attached to it may at all times preserve a vertical position. The regulator *B*, the graduated tube *C*, and the gauge *D*, are of glass, connected by iron joints *ppp*, and further strengthened by brass supporters *iiii:* jointed brass rods *fh*, *fh*, by which the globe *E* swings, are also fastened to the magazine. The waste pipe *nnn*, and the other tubes *mmm*, *ooo*, are made of the elastic gum, which has been found to be impenetrable to air.

In the bottom of the magazine, at the joint *p*, there is an aperture communicating with the conduit *k*; it is rather larger than the aperture *j* in the regulator *B*; and as the waste pipe continually carries off the excess of its volume, there will be always the same depth of mercury maintained in *B* to regulate the issue through *j* into the gauge *D*.

Experience will determine the most suitable substance for mercury to run through. Trial will be made of ruby, which it is apprehended has neither affinity to mercury, nor will be liable to any sensible influence from temperature or friction. By the elevation or depression of the hollow screws in which the apertures are fixed, they receive their necessary adjustments; the lower one *j* being set to fill the gauge *D* in 24 hours, when the gauge and its contents are at the temperature of 62°.

As it may be assumed that mercury expands uniformly throughout the small range of temperature, to which the interior of a ship is ever exposed; (supposing this range to be from 20° to 110°,) tables for immediate use have been calculated on this principle, from data obligingly supplied by Mr. John Dalton, of Manchester, to show the contents, and, consequently, the corresponding value in time of the gauge *D*, at every tenth of a degree of the thermometer. This minute temperature is indicated by the thermometer *M*, which

which has been graduated according to Fahrenheit, and is made to resemble, as much as possible, the gauge *D*, in every circumstance which might affect its result: the scale is great, being about $\frac{1}{4}$ of an inch to a degree, which again is divided into tenths; and the bulb is kept immersed in a body of mercury, contained in the bottom of the glass case, which is to be closed at the same time as the machine, and like it to be well secured from the intrusion of the external atmosphere. In the drawing but little of this thermometer can be seen.

Much of the accuracy of the machine will depend on the tube *C*, which communicates with the regulator and the gauge. It is about six inches in length, of considerable thickness, and its interior capacity such, that the volume from *j* is nearly three minutes in filling it. On its front there are marked small segments of circles 1, 2, 3, 4, 5, 6, 7, 8; directly on the opposite side there are as many other segments, corresponding with the same, each to each. The sight of the observer must be directed so, that by the reflection of the glass, two corresponding segments appear a perfect circle. When the transit of the rising column of mercury interrupts this circle, the instant shown by a second watch is written down by an assistant, in the same way as is usual in taking celestial altitudes or distances. This transit of the column of mercury can be observed to the fraction of a second of time.

The cocks *y* and *z*, are both air and mercury tight, and are so connected by the parallel bars, that the one cannot move independently of the other. When the ivory handle *N* is up, as represented in the drawing, the lower cock *z* is shut, and *y* is open. By putting the handle down, *y* closes, and *z* opens; by which simple movement, having previously lowered the elastic tube from the hook *W*, the mercury in the gauge *D*, discharges itself into the globe *E*, from whence it is restored to the magazine AA, by drawing a rope fastened to the ring *R*, and passing through a pulley fixed above in the deck. In this operation the globe *E*, attached to the rods *h h*, ascends, forming a circular sweep, and carrying with it the rods *h h*, which turn upon their joints *f f*. When the globe *E* has been raised in this manner to the necessary height, the mercury runs from it into the magazine AA, through the pipe *ooo*; which of course is then elevated. The globe *E* being emptied, it must be let gradually down again to its place. A couple of prongs *s* and *t*, attached to the framing at the bottom of the gauge *D*, embrace the rods by which it is hung, and

 lock

lock it securely; so that while locked it can have no motion independent of the machine.

The bent tube in the regulator *B* is an air pipe, for the purpose of maintaining a free circulation of the air contained in the apparatus, whether atmospheric or any other; should it be found expedient to close the machine without adopting the Torricellian vacuum. A similar tube is fixed in the magazine, and for the same purpose.

With this knowledge of the several parts, the following will be sufficient instructions for conducting the operation on ship-board:

On leaving a known meridian, unhook the tube *mmm*, and put down the ivory handle *N*. While the mercury is running out of the gauge into the globe, be careful to compare, with the time of the day, the instants of the transit of the rising column of mercury over the segments 1, 2, 3, 4, &c. Having recorded these observations, and the gauge being perfectly empty, put up the ivory handle, raise the globe *E*, and let the mercury that is in it run into the magazine *AA*.

On the morrow when the gauge is full, and the mercury has appeared rising above the cock *y*, put down the handle *N* to cut off the communication, and unhook the elastic tube as before. Now carefully observe again the column's transit over the segments 1, 2, 3, &c.; and together with these observations write down the height of the large thermometer *M*. The gauge being empty, put up the ivory handle, and restore the mercury in the globe *E* to the magazine *AA*. By reference to the tables showing the capacity of the gauge at every tenth of a degree, find against the temperature, which was noted down, the time that has elapsed since the last operation; which, compared with the ship's time, gives the longitude sought, or the ship's departure from the meridian of the preceding day. Record this time in your book of reckoning, and proceed from day to day in the same manner.

If the operator should be too late, and find the graduated tube *C* filled, he will have lost the opportunity of observing his time for that day. The chamber *G*, which the aperture *j* would be about an hour and a half filling, will secure the instrument from going down; if, in allusion to a watch, I may be permitted the expression. On such an occasion it will be only requisite to unhook the tube, put down the ivory handle, and register the time found in the tables to correspond with the height of the large thermometer. Then, when the gauge is empty, put up the ivory handle,

handle, and go through the usual process of restoring the mercury in the globe to the magazine. This omission (it is to be remarked) occassions no error, only the next day's observation becomes a reckoning for two days.

LXXII. *Further Experiments and Observations on the Influence of the Brain on the Generation of Animal Heat. By* B. C. Brodie, *Esq. F.R.S. Communicated to the Society for promoting the Knowledge of Animal Chemistry, and by them to the Royal Society**.

[Continued from vol. xxxvii. page 452.]

In the Croonian Lecture for the year 1810, I gave an account of some experiments, which led me to conclude that the production of animal heat is very much under the influence of the nervous system. Some circumstances which I have since met with, illustrate this subject, and seem to confirm the truth of my former conclusions.

In an animal which is under the influence of a poison that operates by disturbing the functions of the brain, in proportion as the sensibility becomes impaired, so is the power of generating heat impaired also.

If an animal is apparently dead from a poison of this description, and the circulation of the blood is afterwards maintained by means of artificial respiration, the generation of heat is found to be as completely destroyed, as if the head had been actually removed.

Under these circumstances, if the artificial respiration is kept up until the effects of the poison cease, as the animal recovers his sensibility, so does he also recover the power of generating heat; but it is not till the nervous energy is completely restored, that heat is produced in sufficient quantity to counteract the cold of the surrounding atmosphere †.

In the experiments formerly detailed, as well as in those just mentioned, I observed that the blood underwent the usual alteration of colour in the two systems of capillary vessels, while carbonic acid was evolved from the lungs at

* From the Philosophical Transactions for 1812, part ii.

† The poison employed in this experiment should be the essential oil of almonds, or some other, the effects of which speedily subside. If the woorara is employed, so long a time elapses before the poison ceases to exert its influence, that it becomes necessary that the experiment should be made in a high temperature, otherwise the great loss of heat which takes place, is sufficient to prevent recovery.

 each

each expiration; and hence I was led to believe, that the respiratory function was performed nearly as under ordinary circumstances, and that the usual chemical changes were produced on the blood. It appeared, however, desirable to obtain some more accurate knowledge on this point, and I have therefore instituted a series of experiments, for the purpose of ascertaining the relative quantities of air consumed in breathing, by animals in a natural state, and by animals in which the brain has ceased to perform its office, and I now have the honour of communicating an account of these experiments to the Society.

It has been shown, by Messrs. Allen and Pepys, first, that every cubic inch of carbonic acid requires exactly a cubic inch of oxygen gas for its formation*; secondly, that when respiration is performed by a warm-blooded animal in atmospheric air, the azote remains unaltered, and the carbonic acid exactly equals, volume for volume, the oxygen gas which disappears†.

There is therefore reason to believe, that the watery vapour, which escapes with the air in expiration, is not formed from the union of hydrogen with oxygen in the lungs, but that it is exhaled from the mucous membrane of the mouth and pharynx, resembling the watery exhalation which takes place from the peritonæum, or any other internal surface when exposed; and this conclusion appears to be fully confirmed by the experiments of M. Magendie, lately communicated to the National Institute of Paris.

These circumstances are of importance in the present communication, which they render more simple, as they show, that in order to ascertain the changes produced on the air in respiration, it is only necessary to find the quantity of carbonic acid given out from the lungs. This becomes an exact measure of the oxygen consumed, and the azote of the air and the watery vapour expired need not be taken into the account.

For the purpose of examining the changes produced on the air, by animals breathing under the different circumstances above mentioned, I contrived the apparatus which is represented in the annexed Plate.

Description of the Apparatus. (Plate XI.)

A. Is a wooden stand, in which is a circular groove $\frac{3}{4}$ of an inch in depth, and the same in width.

B. Is a bell-glass, the rim of which is received in the circular

* Phil. Trans. 1807, part ii.

† Phil. Trans. 1808, part ii. Ibid. 1809, part ii.

circular groove of the wooden stand. In the upper part of the bell-glass is an opening, admitting a tube connected with the bladder C.

D. Is a bottle of elastic gum, having a brass stop-cock E connected with it.

F. Is a silver tube, of which one end is adapted to receive the tube of the stop-cock E, while the other extremity, making a right angle with the rest of the tube, passes through a hole in the wooden stand, and projects into the cavity of the bell-glass, where it makes a second turn also at a right angle, and becomes of a smaller diameter. In the upright part of the tube is an opening G.

The tubes are made perfectly air-tight, where connected with each other, and with the rest of the apparatus, and the circular groove is filled with quicksilver.

The capacity of the bell-glass, allowance being made for the rim, which is received in the groove with the quicksilver, is found to be 502 cubic inches. The capacity of the gum-bottle is 52 cubic inches, and in the calculations after the experiments two cubic inches have been allowed for the air contained in the different tubes, and for the small remains of air in the bladder after being nearly emptied by pressure.

Mode of using the Apparatus.

In order to ascertain the quantity of air consumed under ordinary circumstances, the animal was placed on the stand under the bell-glass, the bladder being emptied by pressure, and the gum-bottle being distended with atmospheric air. During the experiment, by pressing occasionally on the gum bottle, the air was forced from it into the bell-glass. On removing the pressure, the gum-bottle became filled by its own elasticity with air from the bell-glass. Thus the air was kept in a state of agitation, and the dilatation of the bladder prevented the air being forced through the quicksilver under the edge of the bell-glass. At the end of the experiment, the gum-bottle was completely emptied by pressure, and allowed to be again filled with air from the bell-glass; this was repeated two or three times, and the air in the bottle was then preserved for examination. The proportion of carbonic acid being ascertained, and the capacities of the different parts of the apparatus, and the space occupied by the animal being known, the total quantity of carbonic acid formed, and consequently of oxygen consumed, was easily estimated.

When the experiment was made on an animal in whom the functions of the brain were destroyed, and in whom therefore voluntary respiration had ceased, the narrow extremity of the tube was inserted into an artificial opening in the trachea, and the animal being placed under the bell-glass, the lungs were inflated at regular intervals, by means of pressure made on the gum-bottle. The tube being smaller than the trachea, the greater portion of the air in expiration escaped by the side of the tube into the general cavity of the bell-glass, while the gum-bottle filled itself by its own elasticity with air through the opening G. At the end of the experiment, a portion of air was preserved for examination, and the quantity of carbonic acid was estimated in the way already described.

The animals employed in these experiments were of the same species, and nearly of the same size. Attention to these circumstances was judged necessary, that the results might be as conclusive as possible. The chemical examination of the air was made by agitating it in a graduated measure over quicksilver, with a watery solution of potash. My friend Mr. Brande gave me his assistance in this part of the present investigation, as he had done on many former occasions. It will be observed, that in estimating the proportion of carbonic acid, no allowance has been made for that contained in the atmospheric air; first, because the quantity is so small that the omission can occasion no material error; and secondly, because the object is to ascertain, not so much the absolute as the relative quantities of carbonic acid evolved by animals breathing under different circumstances.

The experiments which I shall first notice, were made on the respiration of animals in a natural state.

Experiment 1.—Thermometer 65°, barometer not noted.

A young rabbit was allowed to remain under the bell-glass during 30 minutes. The respired air at the end of this time was found to contain $\frac{1}{20}$ of carbonic acid.

It was ascertained that the rabbit occupied the space of 50 cubic inches.

The capacity of the bell-glass = 502 cubic inches.

That of the gum-bottle 52 cubic inches.

The air in the tubes and bladder = 2 cubic inches.

$$\text{Then } \frac{502 + 52 + 2 - 50}{20} = \frac{506}{20} = 25{\cdot}3.$$

The rabbit therefore in 30 minutes gave out 25·3 cubic inches of carbonic acid, and consumed the same quantity of oxygen gas, which is at the rate of 50·6 in an hour.

Experiment 2.—Thermometer 65°, barometer 30·1 inch.

A some-

A somewhat smaller rabbit was allowed to remain under the bell-glass during 30 minutes. The respired air contained $\frac{1}{18}$ of carbonic acid. The animal occupied the space of 48 cubic inches.

$$\frac{502 + 52 + 2 - 48}{18} = \frac{508}{18} = 28{\cdot}22.$$

The carbonic acid evolved was therefore equal to 28·22 cubic inches in half an hour, which is at the rate of 56·44 cubic inches in an hour.

Experiment 3.—Thermometer 64°, barometer 30·2 inch. A young rabbit, occupying the space of 48 cubic inches, was allowed to remain under the bell-glass during the same period as in the two former instances. The respired air contained $\frac{1}{18}$ of carbonic acid.

$$\frac{502 + 52 + 2 - 48}{18} = \frac{508}{18} = 28{\cdot}22.$$

The results of this were therefore precisely the same as those of the last experiment.

These experiments were made with great care. The animals did not appear to suffer any inconvenience from their confinement, and their temperature was unaltered.

The next order of experiments were made for the purpose of ascertaining the quantity of air consumed by animals in which the circulation of the blood was kept up by means of artificial respiration, after the brain had ceased to perform its functions.

Experiment 4.—Thermometer 65°, barometer not noted. Having procured two rabbits of the same size and colour, I divided the spinal marrow in the upper part of the neck of one of them. An opening was made in the trachea, and the lungs wese inflated at first by means of a small pair of bellows. Two ligatures were passed round the neck, one in the upper and the other in the lower part, behind the trachea. The ligatures were drawn tight, including every thing but the trachea; and the nerves, vessels, and other soft parts between them were divided with a bistoury. Eight minutes after the division of the spinal marrow, the thermometer in the rectum had sunk to 97°. The animal was placed under a bell-glass, and the lungs were inflated by pressing on the gum-bottle about 50 times in a minute. When this process had been continued for 30 minutes, a portion of air was preserved for examination. The heart was found acting regularly, but slowly, the thermometer in the rectum had fallen to 90°.

The second rabbit was killed by dividing the spinal mar-

row

row about the same time when the experiment was begun on the first rabbit. Being in the same temperature, the time was noted when the thermometer in the rectum had fallen to 97°, and it was placed under another bell-glass, that it might be as nearly as possible under the same circumstances with the first rabbit. At the end of 30 minutes the thermometer in the rectum had fallen from 97 to 91*.

The air respired by the first rabbit contained $\frac{1}{25}$ of carbonic acid. The bulk of the rabbit was found = 50 cubic inches.

$$\frac{502 + 52 + 2 - 50}{25} = \frac{506}{25} = 20{\cdot}24.$$

20·24 cubic inches of carbonic acid were therefore extricated in 30 minutes, which is at the rate of 40·48 cubic inches in an hour.

The carbonic acid given out in the same space of time was less than in the former experiments; but it is to be observed, first, that in consequence of the ligatures the extent of the circulation was diminished; secondly, that in this instance one of the ligatures accidentally slipped, and an ounce of blood was lost in the beginning of the experiment.

As it was desirable to avoid any circumstances which might occasion a difference in the results, in the subsequent experiments I employed animals which had been inoculated with the poison of woorara, or the essential oil of almonds; by which means, while the functions of the brain were completely destroyed, the extent of the circulation was undiminished, and all chance of accidental hæmorrhage was avoided.

Experiment 5.—Thermometer 65°, barometer 29·8 inch.

Two rabbits were procured, each occupying the space of 45 cubic inches. They were both inoculated with the woorara poison.

The first rabbit was apparently dead in nine minutes after the application of the poison; but the heart continued to act. The lungs were inflated for about two minutes, by means of a pair of bellows, when the thermometer in the rectum was observed to stand at 98°. The animal was placed under the bell-glass, and artificial respiration was produced by means of pressure on the gum-bottle, as in the last experiment. At the end of 30 minutes, a portion of

* In measuring the heat of the rectum in these experiments, care is necessary that the thermometer should always be introduced to exactly the same distance from the external parts, otherwise no positive conclusion can be drawn relative to the loss of heat, as the more internal parts retain their heat longer than the superficial.

air

air was preserved for examination. The thermometer in the rectum had fallen to 91°. The heart still acted with regularity and strength.

The second rabbit died in a few minutes after the inoculation. The time was noted when the thermometer in the rectum had fallen to 98°, and he was placed under a bell-glass. At the end of 30 minutes, the thermometer in the rectum had fallen to 92°.

The air respired by the first rabbit contained $\frac{1}{20}$ of carbonic acid.

$\frac{502 + 52 + 2 - 45}{20} = \frac{511}{20} = 25{\cdot}55$ cubic inches of carbonic acid evolved in 30 minutes, which is at the rate of 51·1 cubic inches in an hour.

Experiment 6.—Thermometer 66°, barometer 30·1 inch.

Two rabbits, each occupying the space of 48 cubic inches, were inoculated with woorara.

In one of them, when apparently dead, the circulation was kept up by means of artificial respiration. He was placed in the apparatus under the bell-glass, and the lungs were inflated from 50 to 60 times in a minute. At this time the thermometer in the rectum stood at 97°. At the end of 35 minutes, a portion of air was preserved for examination. The thermometer had now fallen to 90°. The heart was still acting regularly.

The second rabbit was allowed to lie dead. When the thermometer in the rectum had fallen to 97°, he was placed under another bell glass. At the end of 35 minutes, the thermometer had fallen to 90°·5.

The air respired by the first rabbit contained $\frac{1}{16}$ of carbonic acid.

$\frac{502 + 2 + 52 - 48}{16} = \frac{508}{16} = 31.75$ cubic inches of carbonic acid evolved in 35 minutes, which is at the rate of 54·43 cubic inches in an hour.

Experiment 7.—Thermometer 60°, barometer 30·2 inch.

The experiment was repeated on a rabbit which had been inoculated with the essential oil of almonds. When he was placed under the bell-glass, the thermometer in the rectum stood at 96°. In a few minutes he gave signs of sensibility, and made efforts to breathe; but as these efforts were at long intervals, the artificial respiration was continued. In half an hour he breathed spontaneously 40 times in a minute. The thermometer in the rectum had fallen to 90°.

The air being examined, was found to contain $\frac{1}{16}$ of carbonic acid. The

The rabbit occupied the space of 47 cubic inches.

$\frac{502 + 52 + 2 - 47}{18} = \frac{509}{18} = 28{\cdot}275$ cubic inches of carbonic acid evolved in 30 minutes, which is at the rate of 56·55 cubic inches in an hour.

The animal lay as if in a state of profound sleep. At the end of two hours and twenty minutes, from the time of the poison being applied, the thermometer in the rectum had fallen to 79°, and he was again apparently dead; but the heart still continued acting, though feebly, and its action was kept up for 30 minutes longer by means of artificial breathing, when the thermometer had fallen to 76°. The carbonic acid evolved during these last 30 minutes amounted to nearly 13 cubic inches.

From the precautions with which these experiments were made, I am induced to hope that there can be no material error in their results. They appear to warrant the conclusion, that in an animal in which the brain has ceased to exercise its functions, although respiration continues to be performed, and the circulation of the blood is kept up to the natural standard, although the usual changes in the sensible qualities of the blood take place in the two capillary systems, and the same quantity of carbonic acid is formed as under ordinary circumstances; no heat is generated, and (in consequence of the cold air thrown into the lungs) the animal cools more rapidly than one which is actually dead.

It is a circumstance deserving of notice, that so large a quantity of air should be consumed by the blood passing through the lungs, when the functions of the brain, and those of the organs dependent on it, are suspended. Perhaps it is not unreasonable to suppose, that by pursuing this line of investigation we may be enabled to arrive at some more precise knowledge respecting the nature of respiration, and the purposes which it answers in the animal œconomy. It would however be foreign to the plan of the present communication to enter into any speculations on this subject, and I shall therefore only remark, that the influence of the nervous system does not appear to be necessary to the production of the chemical changes which the blood undergoes in consequence of exposure to the air in the lungs*.

The

* This conclusion is directly contrary to that deduced by M. Dupuytren, from a series of curious experiments, made with a view to ascertain the effects which follow the division of the nerves of the par vagum, and it is an object of some importance in the present investigation, to ascertain in what

The facts now, as well as those formerly adduced, go far towards proving, that the temperature of warm-blooded animals is considerably under the influence of the nervous system; but what is the nature of the connection between them? whether is the brain directly or indirectly necessary to the production of heat? these are questions to which no answers can be given, except such as are purely hypothetical. At present we must be content with the knowledge of the insulated fact: future observations may, perhaps, enable us to refer it to some more general principle.

We have evidence, that when the brain ceases to exercise its functions, although those of the heart and lungs continue to be performed, the animal loses the power of generating heat. It would, however, be absurd to argue from this fact, that the chemical changes of the blood in the lungs are in no way necessary to the production of heat, since we know of no instance in which it continues to take place after respiration has ceased.

It must be owned, that this part of physiology still presents an ample field for investigation.

Of opinions sanctioned by the names of Black, Laplace, Lavoisier, and Crawford, it is proper to speak with caution and respect; but without trespassing on these feelings, I

what manner the apparently opposite facts, observed by M. Dupuytren and myself, are to be reconciled with each other.

It was observed by this physiologist, that in an animal, in which both the nerves of the par vagum are divided, the blood returned from the lungs has a darker colour than natural, and that the animals, on whom this operation is performed, die sooner or later with symptoms of asphyxia, notwithstanding the air continues to enter the lungs; and hence he concludes, that the changes which are produced on the blood in respiration are not the result of a mere chemical process, but are dependent on the nervous influence, and cease to take place when the communication between the lungs and the brain is destroyed.

M. Provençal, in prosecuting this inquiry, ascertained that the animals subjected to this experiment give out less carbonic acid than before

M. Blainville observed, that the frequency of the inspirations is much diminished; and M. Dumas restored the scarlet colour of the arterial blood by artificially inflating the lungs, and from these and other circumstances he has arrived at conclusions very different from those of M. Dupuytren.

My own observations exactly correspond with those of MM. Dumas and Blainville. After the nerves of the par vagum are divided, a less quantity of carbonic acid is evolved, the inspirations are much diminished in frequency, and the blood in the arteries of the general system assumes a darker hue; but its natural colour may be restored by artificially inflating the lungs, so as to furnish a greater supply of air to the blood circulating through them.

We may suppose that, on the division of these nerves, the sensibility of the lungs is either extremely impaired, or altogether destroyed, so that the animal does not feel the same desire to draw in fresh air: in consequence his inspirations become less frequent than natural, and hence arise the phænomena produced by this operation.

may

may be allowed to say, that it does not appear to me that any of the theories hitherto proposed afford a very satisfactory explanation of the source of animal heat.

Where so many and such various chemical processes are going on, as in the living body, are we justified in selecting any one of these for the purpose of explaining the production of heat?

To the original theory of Dr. Black there is this unanswerable objection, that the temperature of the lungs is not greater than that of the rest of the system. To this objection the ingenious and beautiful theory of Dr. Crawford is not open; but still it is founded on the same basis with that of Dr. Black, "the conversion of oxygen into carbonic acid in the lungs;" and hence it appears to be difficult to reconcile either of them with the results of the experiments which have been related.

It may perhaps be urged, that as in these experiments the secretions had nearly if not entirely ceased, it is probable that the other changes which take place in the capillary vessels had ceased also; and that, although the action of the air on the blood might have been the same as under ordinary circumstances, there might not have been the same alteration in the specific heat of this fluid, as it flowed from the arteries into the veins. But on this supposition, if the theory of Dr. Crawford be admitted as correct, there must have been a gradual but enormous accumulation of latent heat in the blood, which we cannot suppose to have taken place without its nature having been entirely altered. If the blood undergoes the usual change in the capillary system of the pulmonary, it is probable that it must undergo the usual change in the capillary system of the greater circulation also, since these changes are obviously dependent on and connected with each other. The blood in the aorta and pulmonary veins was not more florid, and that in the vena cava and pulmonary artery was not less dark-coloured, than under ordinary circumstances. We may moreover remark, that the most copious secretions in the whole body are those of the insensible perspiration from the skin, and of the watery vapour from the mouth and fauces, and the effect of these must be to lower rather than to raise the animal temperature. Under other circumstances also the diminution of the secretions is not observed to be attended with a diminution of heat. On the contrary, in the hot fit of a fever, when the scanty dark-coloured urine, dry skin and parched mouth indicate that scarcely any secretions are taking place, the temperature of the

the body is raised above the natural standard, to which it falls when the constitution returns to its natural state, and the secretions are restored.

It has been observed, by a distinguished chemist, that "the experiments to determine the specific heat of the blood are of so very delicate a nature, that it is difficult to receive them with perfect confidence *." The experiments of Dr. Crawford for this purpose were necessarily made on blood out of the body, and at rest. Now, when blood is taken from the vessels, it immediately undergoes a remarkable chemical change, separating into a solid and a fluid part. This separation is not complete for some time; but whoever takes the pains to make observations on the subject, can hardly doubt that it begins to take place immediately on the blood being drawn. Can experiments on the blood, under these circumstances, lead to any very satisfactory conclusions, respecting the specific heat of blood circulating in the vessels of the body? The diluting the blood with large quantities of water, as proposed by Dr. Crawford, does not altogether remove the objection; for this only retards, it does not prevent coagulation; and some time must, at any rate, elapse, while the blood is flowing and the quantity is being measured, during which the separation of its solid and fluid parts will have begun to take place.

More might be said on this subject; but I feel anxious to avoid, as much as possible, controversial discussion. It is my wish not to advance opinions, but simply to state some facts which I have met with in the course of my physiological investigations. These facts, I am willing to hope, possess some value; and they may perhaps lead to the development of other facts of much greater importance. Physiology is yet in its infant state. It embraces a great number and variety of phænomena, and of these it is very difficult to obtain an accurate and satisfactory knowledge; but it is not unreasonable to expect, that by the successive labours of individuals, and the faithful register of their observations, it may at last be enabled to assume the form of a more perfect science.

* Thomson's History of the Royal Society, p. 129.

LXXIII. *On*

LXXIII. *On a New Œconomical Lamp.*

To Mr. Tilloch.

Sir, In the last number of Nicholson's Journal there is a figure, and a long description, of an *œconomical* lamp, by L. O. C. Like Argand's lamp, it has an air tube through the centre of it, and the flame is surrounded by a glass chimney; but the chimney is very short, and the flame arises from eight small wicks that are placed at equal distances from one another, in a circle round the air-tube. The small tubes which contain the wicks are not large enough in diameter to receive more than four threads of cotton. The inventor compared his lamp with an Argand's lamp, by heating a given quantity of water with each, and found them equal in power, while his own consumed not more than half so much oil as the other!—I have had one of these new lamps constructed, and with it have performed the following experiments:

Experiment 1.—The two shadows of a cylinder of wood were equally strong, when an Argand lamp was fourteen feet, and the new lamp only seven feet four inches, distant from the white plane on which the shadows were exhibited.

Experiment 2.—A dish of water, having a thermometer in it, was placed an inch above the glass chimney of the Argand lamp: in 73 seconds the temperature of the water was elevated from 55° to 75°. The dish, containing the same quantity of water at 55°, was placed an inch above the chimney of the new lamp, and the temperature was not raised to 75° in less than 165 seconds! The chimney of the new lamp was three inches long, and that of the Argand lamp four inches and a half. The new lamp seemed to act as powerfully without the glass as with it.

Experiment 3.—Imagining that the wicks of the new lamp might have a better effect if ranged in a smaller circle, they were accordingly altered, and again used, but this time with a glass chimney only one inch high; for a longer chimney extinguished the lights. The two shadows were equal when this lamp was eight feet one inch distant, and Argand's lamp 14 feet.

Experiment 4.—The Argand lamp raised the temperature of a given quantity of water 10° of Fahrenheit in 58″ of time: the other lamp produced an equal effect in 75″, the chimneys in both cases being placed at the same distance below the dish of water. But when the flame of the new lamp

lamp was placed as far below the water to be heated as the flame of the other had been, the new lamp had not an equal effect in less than 188″.

Experiment 5.—The two lamps being weighed, it was found that the oil consumed by them in the same length of time was nearly as 10 to 13, the Argand lamp consuming the most.

I am, sir,
Your obedient servant,
Cirencester, Dec. 1, 1812. FRANCIS KERBY.

P. S.—Can any of your readers inform me what liquid or solid substance has the property of absorbing the vapour of ether?

LXXIV. *On some Combinations of Phosphorus and of Sulphur, and on some other Subjects of Chemical Inquiry.* *By Sir* HUMPHRY DAVY, *Knt. LL.D. Sec. R.S.**

1. INTRODUCTION.

In this paper, I shall do myself the honour of laying before the Society the results of some experiments on phosphorus and sulphur, which establish the existence of some new compounds, and which offer decided evidences in favour of an idea that has been for some time prevalent amongst many enlightened chemists, and which I have defended in former papers published in the Philosophical Transactions; namely, that bodies unite in definite proportions, and that there is a relation between the quantities in which the same element unites with different elements.

I shall not enter into a minute detail of the methods of experimenting that I employed; I shall confine myself to general statements of the facts. The common manipulations of chemistry are now too well known to require any new illustrations: and to dwell upon familiar operations would be to occupy unnecessarily and tediously the time of this learned body.

2. *Of some Combinations of Phosphorus.*

In a paper read before the Royal Society in 1810, I have described the mutual action of phosphorus and oxymuriatic gas, or chlorine. I have noticed two compounds which appear to be distinct and peculiar bodies, formed by the union of the gas and the inflammable substance. One is

* From Philos. Trans. for 1812, part ii.

solid, white, and crystalline in its appearance; easily volatile, and capable of forming a fixed infusible substance by uniting with ammonia. The other is fluid, limpid as water, and, as I have since found, of specific gravity 1·45; it produces dense fumes by acting upon the water of the atmosphere, and when exposed to the atmosphere gradually disappears, leaving no residuum.

The composition of the white sublimate is very easily ascertained by synthetical experiments, such as I have described on a former occasion in the Transactions. By employing chlorine dried by muriate of lime, in great excess, and making the experiments in exhausted vessels, and admitting solution of chlorine to ascertain the quantity of gas absorbed, I have ascertained that three grains of phosphorus unite with about 20 grains of chlorine to form the sublimate.

If the phosphorus be in great excess in the experiment of its combustion in chlorine, some of the liquor is formed with the sublimate; but to obtain it in considerable quantities, phosphorus should be passed in vapour through heated powdered corrosive sublimate. A bent glass tube may be used for the process, and the liquor condensed in a cold vessel connected with the tube.

I have not been able to determine its composition by synthetical experiments; but by pouring it gradually into water, suffering the water to become cool after each addition of the liquor, and then precipitating the solution by solution of nitrate of silver, I have ascertained the quantity of chlorine and of phosphorus it contains. 13·6 grains, treated in this way, afforded 43 grains of horn silver.

It is evident from this analysis, compared with the result of the synthetical experiments on the sublimate, that, the quantity of phosphorus being the same, the sublimate contains double as much chlorine as the liquor.

When phosphorus is heated in the liquor, a portion is dissolved, and it then when exposed to the atmosphere leaves a film of phosphorus, which when the liquor is thrown on paper usually inflames: a substance of this kind was first procured by MM. Gay Lussac and Thenard, by distilling phosphorus and calomel together; and it may be produced in the experiment with corrosive sublimate, if sufficient heat be used to sublime the phosphorus, or if there be not an excess of the corrosive sublimate. I have made no experiments in order to ascertain the quantity of phosphorus the liquor will dissolve.

When the white sublimate is made to act upon water, it

dissolves

dissolves in it, producing much heat. The solution evaporated affords a thick liquid, which is a solution of pure phosphoric acid, or a hydrat of phosphoric acid.

When the liquor is treated with water in the same way, it furnishes likewise a thick fluid of the consistence of syrup, which crystallizes slowly by cooling, and forms transparent parallelopipedons.

This substance has very singular properties: when it is heated pretty strongly in the air, it takes fire and burns brilliantly, emitting at the same time globules of gas, that inflame at the surface of the liquid. This substance may be called *hydrophosphorous* acid; for it consists of pure phosphorous acid and water. This is proved by the action of ammoniacal gas upon it: when it is heated in contact with ammonia, water is expelled and phosphite of ammonia formed; and it is likewise shown by the results of its decomposition in close vessels, which are phosphoric acid and a peculiar compound of phosphorus and hydrogen.

Ten parts in weight of the crystalline acid I found produced about 8·5 parts of solid phosphoric acid, and the elastic product must of course have formed the remainder of the weight, allowing for a small quantity of the substance not decomposed.

The peculiar gas is not spontaneously inflammable; but explodes when mixed with air, and heated to a temperature rather below 212°.

Its specific gravity appeared from an experiment in which a small quantity of it only was weighed, to be to that of air nearly as 87 to 100. Water absorbed about one-eighth of its volume of this gas. Its smell was disagreeable, but not nearly so fetid as that of common phosphuretted hydrogen.

When it was detonated with oxygen, it was found that three of it in volume absorbed more than five in volume of oxygen, and a little phosphorus was precipitated.

When potassium was heated in contact with it, its volume increased rapidly till it became double, and then no further effect was produced. The potassium was partly converted into a substance having all the characters of phosphuret of potassium; and the residual gas absorbed the same quantity of oxygen by detonation as pure hydrogen. When sulphur was sublimed in the gas over mercury, the volume was likewise doubled; a compound of phosphorus and sulphur was formed, and the elastic fluid produced had all the characters of sulphuretted hydrogen.

It appears from these experiments, that the peculiar gas consists of 4·5 of hydrogen in weight to 22·5 phosphorus;

 and

and its composition being known, it is easy to determine the composition of the hydrophosphorous acid, and likewise the quantity of oxygen required by a given quantity of phosphorous acid to be converted into phosphoric acid; for, for every volume of gas disengaged, a volume of oxygen must have been fixed in the phosphoric acid.

And calculating for 174 grains, 30 parts of oxygen must be fixed in the 150 parts of phosphoric acid, and 20 parts of phosphorus disengaged in combination with four parts of hydrogen; and on the idea of representing the proportions in which bodies combine by numbers, if hydrogen be considered as unity, and water as composed of two proportions of hydrogen, 2, and one of oxygen 15*, phosphorus will be represented by 20.

When the compounds of chlorine and phosphorus are acted on by a small quantity of water, muriatic acid gas is disengaged with violent ebullition, the water is decomposed, and it is evident that for every volume of hydrogen disengaged in combination with the chlorine, half a volume of oxygen must be combined with the phosphorus†; and the products of the mutual decomposition of water, and the phosphoric compounds of chlorine, are merely the phosphoric acid from the sublimate and the phosphorous acid from the liquor, and muriatic acid gas; so that, the quantity of phosphorus being the same, it is evident that phosphoric acid must contain twice as much oxygen as phosphorous acid, which harmonizes with the results of the decomposition of hydrophosphorous acid. For supposing water to be composed of two proportions of hydrogen, and one of oxygen, and the number representing it 17; then 174 parts of hydrophosphorous acid must consist of two proportions; 34 parts of water, and four proportions of phosphorous acid, containing 80 of phosphorus and 60 of oxygen; and three proportions of phosphoric acid must be formed, containing three proportions of phosphorus 60, and six proportions of oxygen 90, making 150.

It is scarcely possible to imagine more perfect demonstrations of the laws of definite combination, than those furnished in the mutual action of water and the phosphoric compounds. No products are formed except the new combinations; neither oxygen, hydrogen, chlorine, nor

* Supposing 100 cubical inches of the gas to weigh 27 grains: 27—4·5 the weight of 200 cubical inches of hydrogen = 22·5 grains.

† This mode of estimation is the same as that I have adopted on a former occasion, except that the number representing oxygen is doubled to avoid a fractional part.

phosphorus

phosphorus is disengaged, and therefore, the ratio in which any two of them combine being known, the ratios in which the rest combine, in these cases, may be determined by calculation.

I converted phosphorus into phosphoric acid, by burning it in a great excess of oxygen gas over mercury in a curved glass tube, and heated the product strongly. I found in several processes of this kind, that for every grain of phosphorus consumed, four cubical inches and a half of oxygen gas were absorbed; which gives phosphoric acid as composed of 20 of phosphorus to 30·6 of oxygen; a result as near as can be expected to the results of the experiments on the sublimate and the hydrophosphorous acid.

Unless the product of the combustion of phosphorus is strongly heated in oxygen, the quantity of oxygen absorbed is less, so that it is probable that phosphorous acid is formed, as well as phosphoric acid.

Phosphorous acid is usually described, in chemical authors, as a fluid body, and as formed by the slow combustion of phosphorus in the air; but the liquid so procured is, I find, a solution of a mixture of phosphorous and phosphoric acids. And the vapour arising from phosphorus in the air at common temperatures, is a combination of phosphorous acid and the aqueous vapour in the air, and is not, I find, perceived in air artificially dried.

In this case, the phosphorus becomes covered with a white film, which appears to be pure phosphorous acid, and it soon ceases to shine.

A solid acid, volatile at a moderate degree of heat, may be produced by burning phosphorus in very rare air, and this seems to be phosphorous acid free from water; but some phosphoric acid and some yellow oxide of phosphorus are always formed at the same time.

The peculiar gas differs exceedingly from phosphoretted hydrogen formed by the action of earths and alkalies and phosphorus upon water; for this last gas is spontaneously inflammable, and its specific gravity is seldom more than half as great, and it does not afford more than 1·5 its volume of hydrogen when decomposed by potassium; it differs in its qualities in different cases, and probably consists of different mixtures of hydrogen with a peculiar gas, consisting of two parts of hydrogen and 20 of phosphorus; or it must contain several proportions of hydrogen to one of phosphorus.

I venture to propose the name *hydrophosphoric* gas for the new gas; and according to the principles of nomenclature,

clature, I have proposed in the last Bakerian lecture, that the liquor containing 20 of phosphorus to 67 of chlorine may be called *phosphorane*, and the sublimate *phosphorana*.

3. *Of some Combinations of Sulphur.*

I have shown, in a paper published in the Philosophical Transactions for 1810, that sulphuretted hydrogen is formed by the solution of sulphur in hydrogen, and I have supposed that sulphureous acid, in like manner, is constituted by a solution of sulphur in oxygen. There is always a little condensation of volume in experiments on the combustion of sulphur in oxygen; but this may fairly be attributed to some hydrogen loosely combined in the sulphur; and to the production of a little sulphuric acid by the mutual action of hydrogen, oxygen, and sulphur.

It is only necessary, if these data be allowed, to know the difference between the specific gravity of sulphureous acid gas and oxygen, and sulphuretted hydrogen and hydrogen, to determine their composition.

In the Philosophical Transactions for 1810, page 254, I have somewhat under-rated the weights of sulphuretted hydrogen and sulphureous acid gases: for I have since found, that the cubical inch measures, employed for ascertaining the volumes of gas weighed, were not correct. From experiments which I think may be depended upon, as the weights of the gases were merely compared with those of equal volumes of common air, I found that 100 cubical inches of sulphureous acid gas weighed 68 grains at mean temperature and pressure, and 100 cubical inches of sulphuretted hydrogen 36·5 grains, and the last result agrees very nearly with one given by MM. Gay Lussac and Thenard, and one gained by my brother Mr. John Davy.

If 34, the weight of 100 cubical inches of oxygen gas, be subtracted from 68, it will appear that sulphureous acid consists of equal weights of sulphur and oxygen, an estimation which agrees very nearly with one given by M. Berzelius; and if 2·27, the weight of 100 cubical inches of hydrogen, be subtracted from 36·5, the remainder 34·23 will be the quantity of sulphur in the gas; and the number representing sulphur may be stated as 30; and sulphureous acid as composed of one proportion of sulphur 30, and two of oxygen 30; and sulphuretted hydrogen as composed of one proportion of sulphur and two of hydrogen.

From the experiments of MM. Gay Lussac, it appears that sulphuric acid decomposed by heat affords one volume of oxygen to two of sulphureous acid: from this it would appear

appear to be composed of one proportion of sulphur to three of oxygen. I have endeavoured, in several trials by common heat and by electricity, to combine sulphureous acid gas with oxygen, so as to form a sulphuric acid free from water, but without success; and it is probable that three proportions of oxygen cannot be combined with one proportion of sulphur, except by the intermedium of water. Mr. Dalton has supposed that there is a solid sulphuric acid formed by the action of sulphureous acid gas upon nitrous acid gas. But I find, that when dried sulphureous acid gas and nitrous acid gas are mixed together, there is no action; but by introducing the vapour of water, they form together a solid crystalline hydrat; which when thrown into water gives off nitrous gas, and forms a solution of sulphuric acid.

I have referred, in the Philosophical Transactions, to the combination of chlorine and sulphur. I have been able to form no compound of these bodies, which does not deposit sulphur by the action of water. When sulphur is saturated with chlorine, as in Dr. Thomson's sulphuretted liquor, it appears to contain, from my experiments, only 67 of chlorine to 30 of sulphur.

4. *Some general Observations.*

It is a fact worthy of notice, that phosphoric and sulphuric acids should contain the same quantity of oxygen to the same quantity of inflammable matter; and yet that the oxygen should be combined in them, with such different degrees of affinity. Phosphorous acid has a great tendency to unite with oxygen, and absorbs it even from water; and sulphureous acid can only retain it when water is present.

The relation of water to the composition of many bodies has already occupied the attention of some distinguished chemists, and is well worthy of being further studied: most of the substances obtained by precipitation from aqueous solutions are, I find, compounds of water.

Thus zircona, magnesia, silica, when precipitated and dried at 212°, still contain definite proportions of water. And many of the substances which have been considered as metallic oxides, that I have examined, obtained from solutions, agree in this respect; and their colours and other properties are materially influenced by this combined water.

I shall give an instance. The substance which has been called the white oxide of manganese is a compound of water and the protoxide of manganese, and when heated strongly, it gives off its water and becomes a dark olive oxide.

It has been often suspected, that the contraction of volume produced in the pure earths by heat is owing to the expulsion of water combined with them. The following fact seems to confirm this suspicion, and offers a curious phænomenon.

Zircona, precipitated from its solution in muriatic acid by an alkali, and dried at a temperature below 300°, appears as a white powder, so soft as not to scratch glass. When heated to 700° or 800°, water is suddenly expelled from it, and notwithstanding the quantity of vapour formed, it becomes at the moment red hot. After the process, it is found harsh to the feel, has gained a tint of gray, its parts cohere together, and it is become so hard as to scratch quartz.

LXXV. *On the Construction and Effects of the Pneumatic Tinder-Box.* By Le Bouvier Desmortiers*.

The inflammation of spunk † in the Pneumatic Tinder-box, by the compression of air alone, is a phænomenon with which chance, the father of discovery, has lately enriched natural philosophy. Many have reasoned on its cause; which some consider to be caloric, others electricity; but no one, that I know of, has attempted to support his opinion by experiments. Without bias for any hypothesis, I have made some researches on the construction and effects of the pneumatic tinder-box, the results of which shall be the subject of the present paper. In the first part, I shall consider what relates to the structure of the instrument; in the second, I shall give an account of the experiments that tend to the discovery of the cause of its effects.

I. The first construction of these tinder-boxes was a little faulty, in the piston being commonly 18 or 20 lines long. This was said to be necessary, that the air might not escape when the piston was in action; for, if there were any point not accurately fitted to the inside of the tube, the air escapes, and the spunk does not kindle.

The goodness of the instrument does not depend on the length of the piston, but on the accuracy with which it fills the bore of the tube; with a tube well bored and a piston

* *Journal de Phys.* tom. lxvii.

† Spunk is prepared from agaric, which is first boiled in water; beaten well when dry; steeped in a strong solution of saltpetre; and lastly dried in an oven. If the solution of nitre be too strong, the agaric is loaded with this salt, which retards its inflammation.

of

of six lines, the air will no more pass than with a piston of twenty. Accordingly, for a tube of six inches I have reduced the piston to six lines, which adds an inch to the column of air, and diminishes the friction two-thirds, so that the effect of the tinder-box is more certain, and it is more easily used. With a little dexterity you may kindle the spunk by holding the tube in one hand and pushing the piston with the other, without being obliged to rest it on a table, or any other solid body. Mr. Dumotiez, a skilful maker of philosophical instruments, is so fully convinced of the advantage of short pistons, that he now makes them of these dimensions*.

They should be employed also in the syringes of air guns†, of fountains acting by compressed air, of the apparatus for artificial mineral waters, of fire-engines, which are worked with so much labour, and even of air-pumps. As the shortening the piston is an advantage to the pump, we obtain a greater effect with less labour, and in a shorter time, than with long pistons.

It is essential, too, that the instrument does not leak at the part where the spunk is placed, because there the transient action of inflammation takes place, and a slight emission of air would prevent the effect. But this effect is produced, though the piston does suffer the air in the tube to pass it. To satisfy myself of this, I made the following experiment, at which they who have seen it were greatly surprised.

In the length of the piston I made a groove a quarter of a line broad. The spunk took fire as before. Three other grooves were added successively, opposite one another, so as to divide the piston into equal parts; and still the spunk took fire. When the grooved piston is moved backward and forward in the tube, the air may be heard entering or issuing out; and the friction is so slight, that the effect of the instrument is easily obtained by pushing it with the hand. This kind of piston would be preferable to those that fit accurately, if a solid substance were employed, hard enough to resist the continual friction of the air passing through the grooves, if I may be allowed the expression. The grooves in leather pistons soon alter their shape, and spread so as to allow the air to pass it in too large quantity.

* Mr Gill, of St. James's street, London, makes these instruments of still smaller dimensions.—Edit.

† In the air guns of Germany, which are the best we know, the piston of the syringe is extremely short.

The piston with four grooves acting very well, I made one with a single groove, of dimensions equal to the other four, and what I foresaw actually took place: there was no inflammation. The following are the reasons of this difference.

The extremity of the grooved piston exhibits the area of a circle, the periphery of which touches the interior edge of the grooves. The column of air contained in the tube rests almost wholly on this base. Only the parts corresponding to the grooves, that are continued through the length of the piston, communicate with the external air. When the piston is pushed with sufficient velocity to kindle the spunk, the parts of the column corresponding to the grooves rush into them with equal velocity; but the friction they experience in passing through such narrow tubes occasions a resistance to their passage, a kind of choking, that suffers only a part to escape, while the column resting on the area of the piston is pushed entirely toward the extremity of the tube, where the spunk to be kindled lies.

In the piston with a single broad groove, the area of the circle, on which the column of air rests, is much smaller, consequently the column itself is less. The resistance the air experiences in passing through the groove is next to nothing; for we hear no noise on moving the piston backward and forward; and as air expands in all directions, when the piston is moved, the column resting on the area of the circle, resting at the same time laterally on that which answers to the groove, it recedes from all the points of contact, and flows entirely through the channel it finds open. It is so true, that it wholly flows out, that the piston, when it touches the extremity of the tube, remains there; while with other pistons a sufficient quantity of air is retained to occasion a spring and repel them.

I think it proper to say a word or two on the quality of the spunk. The driest, softest, and least impregnated with nitre should be chosen. In that of the best quality a piece will not always be found equally good throughout. Some contains a great deal of nitre, and is kindled with more difficulty. This may be known by the cool taste it leaves on the tongue; or by kindling it: for when it has taken fire the nitre melts, and sometimes throws out sparks, that may be dangerous when they spirt out of the instrument, particularly if made with a cock. As it is usual to blow on the spunk, to try whether it be kindled, a spark may be thrown

thrown from it into the eye. This painful accident once happened to me.

They who imagine that electricity kindles the spunk, consider these sparks as an incontrovertible proof of their opinion. I think they are mistaken in this case; yet I must not conceal a fact communicated to me by Mr. Veau-Delaunay, which seems to confirm this opinion, of which he is a partisan. Out of twelve times, when he operated with the instrument without any spunk in it, he saw sparks emitted three times. There are strong reasons, however, for suspecting that electricity is not the cause of the inflammation here. These I shall give in the second part of this paper, concluding the present with an important observation on the construction of pistons.

If we could find an elastic substance sufficiently compact to be turned in a lathe, we should have perfect pistons, that would spring and adapt themselves to the inequalities of the tube, without suffering a bubble of air to escape. I have made some with caoutchouc, softened before the fire, in order to give it a degree of elasticity more obedient to the inequalities of the tube. But on attempting to turn it in a lathe, it bent under the tool. Even the edge of a razor would not take hold of it; so that the piston remained uneven and almost ragged, and yielded like soft wax under the fingers. In this imperfect state it so far prevents the air from escaping, that a column of three inches is sufficient to kindle the spunk; but after a few strokes of the piston the heat dilates it to such a degree, that it cannot be moved without considerable force. If a drop of oil be put on it, it moves easily; but this soon spoils the instrument; for the oil dissolves the caoutchouc, and forms a varnish, which, as the piston grows hot, makes it adhere still more strongly to the sides of the tube.

Might not these inconveniences be avoided, by arming the piston rod with caoutchouc, and covering this with leather? If this process succeeded, it might be applied with advantage to all sorts of pumps.

II. To attain, if possible, a knowledge of the principle of inflammation in the pneumatic tinder-box, four things are to be considered—the materials of the tube, the matter contained in the tube, the materials of the piston, and the friction. Among the materials of the piston I include the grease, with which it is coated, to make it move more easily, and render it fitter to intercept the passage of the air.

In examining the question whether the spunk be kindled by electricity, I consider,

1st, That

1st, That no part of the instrument is insulated; and that insulation is a necessary condition for producing sensible electricity with any of the machines we know. I say machines that we know, because the animal electricity, that manifests itself without insulation, is an exception to our mechanical means, and cannot here be taken into consideration.

2dly, The friction of the piston, which is a greasy body, against a metallic substance, is not calculated to produce electricity.

3dly, Experience demonstrates, that, unless during storms, the atmosphere seldom exhibits any signs of electricity at the height we breathe it; and that we must search for them with instruments in a more elevated region, or when electric clouds are passing over our heads. How then shall we estimate the infinitely small quantity of electric matter in a cubic inch of air, or even less, which the instrument contains?

4thly, It is not without great difficulty that we can kindle spunk with strong electric sparks. I have discharged a large jar on spunk strewed with powdered resin, and it has remained unkindled, though the resin caught fire, and burned entirely away.

As long as the instrument was made with metallic substances only, we were obliged to confine ourselves to the exterior marks of inflammation alone, without being able to assign the true cause, or at least furnish proofs of it. For to guess, is not sufficient in natural philosophy; we must demonstrate, in order to give to facts that degree of certainty which befits science; and this we cannot do here, without seeing what passes at the very point of inflammation.

The means are very simple. Nothing is necessary but to substitute a glass for a metal tube. Those found in the shops being too slight, I applied to Mr. Laurent, the inventor of glass flutes, requesting him to procure me tubes of a similar quality. This artist, as much distinguished by his civility as by his talents, furnished me with three, which I fitted up. The first, eight inches long by eight lines in diameter, did not kindle the spunk. The second, nine inches long by six lines and three-quarters in diameter, kindled it completely. This being destroyed by accident, I tried the third, eight inches long by seven lines in diameter, which succeeded equally well.

When the instrument is made to act, and the spunk kindles, we see a bright flash, that fills the capacity of the

tube;

tube; and this light is so much the more vivid, in proportion as the compression is more rapid. If the compression be less powerful, the spunk does not kindle; but we perceive in the upper part of the tube a light vapour, that falls in undulations on the piston. When this has disappeared, if we draw back the piston, the vapour will re-appear, as long as there is any air in the tube. These effects may be produced several times in succession, merely by pushing the piston with the hand. This vapour is so thin and diaphanous, that it is not perceptible in a strong light. It requires a sort of twilight to see it well.

But whence arises this vapour, and what is its nature? Assuredly it is not furnished by the materials of the instrument; it can only proceed, therefore, from what it contains, from the atmospheric air. Now, according to the present state of our knowledge, the air contains only nitrogen, oxygen, and a very small portion of carbonic acid; all gasiform substances, which are kept in this state by the great quantity of caloric that penetrates them, and are consequently heavier than it*. But in compressing the air contained in the tube, what is the substance that must first give way? Is it not that which is lightest, the caloric, that general solvent, that principle of fluidity and volatilization, which gives wings even to metals to raise themselves in the air? Is then the vapour in question caloric, rendered visible by approximation of its particles, which are compressed by the surrounding air, as air becomes visible in passing through liquids? This idea, which I am far from presenting as a thing proved, acquires more probability from the following experiments.

I substituted hydrogen for common air, and the vapour showed itself as before; but the spunk did not take fire. With carbonic acid gas, and with nitrogen, the effects were the same. The latter, which contained a little nitrous gas, gave a somewhat denser vapour. Oxygen, lightly compressed, yielded a vapour more rare and transient than that of the common air. It had scarcely fallen on the piston, when it rebounded and disappeared. When I compressed

* The air likewise, in its ordinary state, contains twelve grains of water in a cubic foot. This small quantity of water, reduced to the proportion of the quantity of air contained in the apparatus, contributes nothing to its effect; for the heat produced by the friction could at most reduce it to vapour, and in this state it would not kindle the spunk. If the vapour seen in the tube were water in a state of expansion, when it fell on the surface of the piston it would condense there, and appear in the state of a liquid. But the surface of the piston always remains dry, though on moving it the vapour appears and disappears several times.

oxygen

oxygen with a proper force for producing inflammation, the spunk, which commonly takes fire only at the anterior part, was almost burned: yet for this experiment I used a copper instrument, the piston of which lost air so much, that it would no longer kindle spunk with common air.

Perhaps it will be said that the vapour came from the greasy matter on the piston, which adheres to the sides of the tube; and that it is expanded by the heat produced by the friction. To this I answer, in this case, 1st, The vapour should not show itself before the greasy matter is deposited on the sides of the tube; yet it appears at the first stroke of the piston, before the tube becomes greasy. 2dly, It should show itself below the piston, in the part which the piston has left; but, on the contrary, it always shows itself above. 3dly, There is no vapour, when the piston loses much air, if the friction be ever so rapid. 4thly, The vapour should be more apparent, when the piston exerts its friction throughout the whole length of the tube, than when it is confined to a small part of its upper extremity; yet the reverse frequently happens. 5thly, When the air is entirely decomposed no more vapour appears, but it shows itself again, if ever so little fresh air be introduced.

As it was essential to ascertain whether the vapour did not contain an acid principle, I fastened to the surface of the piston, with a little green wax, a piece of muslin dipped in infusion of litmus, and afterwards dried. After twenty strokes of the piston the colour was not changed. I put on a second piece of muslin larger than the first, and the edges of which were loose. This burned all round, without the colour of the rest being altered. Lastly, a third piece, which was wet, experienced no change of colour.

From these experiments it follows, that no acid principle is developed; that all aëriform substances, as well as common air, produce a light vapour: that no other gas, except oxygen and common air, kindles the spunk: that oxygen produces a much more powerful combustion than common air, consequently oxygen acts an important part in the inflammation: that as it can exert its action only when set free by the decomposition of the common air, of which it constitutes a fourth part, it follows, that the air contained in the tube is decomposed by the simple force of compression: that the vapour produced is not owing to the oxygen, since it shows itself equally in gases that contain no oxygen: that this vapour is the effect of some agent common to all gases: and that we may presume it

is

is caloric itself, rendered visible by the sudden approximation of its parts in a small space, where it rises to a temperature that is increased in the oxygen so as to kindle the spunk*.

I am equally induced to believe, since the air (and it is the same with all gases) is decomposed by rapid compression, that the luminous meteors frequently perceived in hurricanes are not always the effects of electricity. I have observed several times, on these occasions, that Saussure's atmospheric electrometer affords no signs of any. I will mention a particular instance, as it occasioned me no less surprise than damage.

In the beginning of the year 1803, being at my country seat, toward evening a violent wind arose, which continued increasing for two hours to such a degree, as to blow down about sixty trees of prodigious size and height in an ornamental plantation. It threw them one upon another in a row, and some of them were broken off. Those that were torn up by the roots brought up the earth with them to the distance of fifteen feet. The clouds flew with extreme rapidity, and I saw flashes of light from them. I raised my electrometer, armed with its conductor two feet long, but the balls still continued in contact.

LXXVI. *Mr.* Hume's *Remarks on Dr.* Roget's *Reply.*

To Mr. Tilloch.

Sir, Dr. Roget having confessed that I did make use of the words "or any other alkali," the question at issue between us becomes more simple; it is merely this,—— Whence are the distinctions between sulphate of copper and nitrate of silver, which prove that one of these salts is *totally* different from the other, when applied for the detection of arsenic?

That there is a total difference, especially when *ammonia* is employed, has been asserted by Dr. Roget;—that there is *no* such dissimilarity in the application of these two tests, to support such an opinion, I shall now endeavour to prove.

* It sometimes happens, that the spunk is turned black without kindling. In this case, as well as when it is kindled, if we draw back the piston in the tube, a dense vapour, that may be smelt, issues out, which is not of the same nature as the former. That shows itself before the inflammation: this always succeeds it. That is the principle of the inflammation: this is a product furnished by the combustion of the spunk, of which it has the smell.

Let

Let us therefore compare the two tests, (for the ammonia is assuredly not to be called a *test* for arsenic) and see if there be any other *essential* difference between them than that which was first noticed by myself, in the Philosophical Magazine, nearly four years ago, in these words :—" The test I propose as a substitute (for sulphate of copper) appears to be more efficacious, inasmuch as it produces a *more copious* precipitate from a given quantity of arsenic."

1. Both these tests have a *metallic* base ; they are applied to the same purpose ; and they admit of the same variations, especially with respect to the *alkaline* agent, in their application for the detection of arsenic.

2. The addition of an alkali, of some kind or other, is as necessary with one test as it is to succeed with the other; and no less caution is required in either of the experiments to avoid an excess of the alkali.

3. Any of the alkaline *earths* will serve with sulphate of copper, as I have proved they will with nitrate of silver. Though I have not made some experiments which I have in contemplation, I suspect the sulphate of copper will produce a more dense and conspicuous precipitate, when the arsenic is combined with *barytes;* if so, this notice will prevent any one arrogating to themselves the discovery of a new test of *this* species.

4. Both the copper and silver tests act exactly upon the same chemical principles, or, in the language of Bergman, by double elective attraction.

5. It is here altogether in my favor that these metallic salts have the same property in respect to *ammonia*, that of forming *triple* or ternary compounds. Of this fact both Dr. Marcet and Dr. Roget seem to have been totally ignorant, before I prescribed the *ammoniaco-nitrate of silver*, which you did me the honor to place among your pages in a late number. Dr. Roget need not, therefore, be surprised that I should become the "panegyrist" of such a finished composition. As we may with the same facility prepare a triple salt with *copper*, to serve as a test for arsenic, I trust, it would be unnecessary to carry this parallel any further, or advance additional arguments to shew that these tests are, *mutatis mutandis*, identical in their application and mode of operating, in respect to *arsenic*.

6. Both these tests agree in producing more *abundant* precipitates; the *silver* is, however, as I have already noticed, by far the most delicate and conspicuous.

Lastly, there is a striking analogy in these tests, in respect to the colour each produces, so that the operator may instantly

instantly discover in what state the arsenical salt exists, whether it be an *arsenite* or *arsenate*: with copper, the first is of a yellowish green, and the arsenate is a bluish green; but with silver, the arsenite is of a *bright yellow*, and the arsenate of a *brick red*. In describing these appearances of the *silver*-test, Dr. R. has copied me nearly verbatim*.

So little indeed do these tests differ in the mode of applying them, and in their habitudes with alkalies and arsenic, that I might have contented myself, when I first announced the *silver*-test, by merely recommending it to the notice of your numerous readers, to be substituted for that of *copper*, as being more eligible.

It is also evident, that the *kind* of alkali was with me a secondary object in the experiments with which I commenced the enquiry; for, in the letter already quoted, I have said, "though my process answer very well with potass, or even with lime-water, I am inclined to prefer the common sub-carbonate of soda."

I must insist, that the name due to the test is to be derived from the metallic salt I employ, that is, any soluble salt of which *silver* is the base; and not from the alkali or alkaline earth, more than from the acid or fluid, all of which the operator may have his choice. In this particular point, Dr. Roget has not done me justice; he might have quoted my test, in the way several respectable authors have done, as the "Nitrate of Silver;" he might have copied Dr. Henry at least as well as Mr. Thomson and some others, and not mock his friend by ascribing to him a new *test* when, take it in the most favourable light, it can only be a *modification*. It is singular that Dr. Roget did not act by me as he has done in quoting Dr. Bostock.—Here he mentions the sulphate of copper, but leaves the reader to guess at the *alkali* which this able philosopher employed.

Dr. Roget has told you that "it was only at the moment" of sending his paper to the press that Dr. Marcet met with the quotation in Dr. Henry's Elements of Chemistry, which led Dr. R. to read my letters; and, you are likewise informed, that Dr. Roget did no more than quote the proper authorities.

Now, in all these documents, where did Dr. Roget find that my process "appears less convenient in its practical application" than any other? That my test might prove so in some hands is not to be doubted; but it was incumbent

* Medic. and Phys. Journ. 1810.

on Dr. Roget to prove its imperfection, previously to his passing such a censure upon it.

No one is more averse to controversy than myself, and this temper I have endeavoured to maintain through the whole of this discussion; as a proof of this, I have constantly endeavoured to render my papers useful to your readers, by dwelling more upon the subject as it concerns chemistry, and less upon that which is connected with my professional character, which, situated as I am, it is my duty to defend; and, in this attempt to improve science, I trust I have so far succeeded as to render this controversy more interesting to your numerous readers than Dr. Roget is willing to allow.

I remain, sir,

Your obedient servant,

Long-Acre, London, Dec. 14, 1812. Jos. Hume.

LXXVII. *Notices respecting New Books.*

Sir Humphry Davy's "Elements of Chemical Philosophy."

[Concluded from p. 307.]

In the absence of all decisive general principles to found a theory of electrical phænomena, Sir H. observes, "It would appear, that in all cases of electrical action, the two electrical states are always coincident, either in different parts of the same body, or in two bodies; and that they are always equal, and capable of neutralizing each other. If a connection be made by a wire, between the positive and negative conductors of the electrical machine, during the time of its action, all electrical effects cease; and to produce a succession of effects, both conductors must be brought near bodies connected with the ground, which gain the opposite state in consequence of what may be called *induction*. When a nonconductor, or imperfect conductor, provided it be a thin plate of matter, placed upon a conductor, is brought in contact with an excited electrical body; the surface, opposite to that in contact, gains the opposite electricity from that of the excited body; and if the plate be removed from the conductor and the source of electricity, it is found to possess two surfaces in opposite states. If a conductor be brought into the neighbourhood of an excited body, the air, which is a nonconductor, being between them; that extremity of the conductor, which is opposite to the excited body, gains the opposite electricity, and the other extremity, if opposite to a body connected

with

with the ground, gains the same electricity, and the middle point is not electrical at all. This is easily proved, by examining the electricity of three sets of gilt pith balls raised on wires on the different parts of the conductor, which is thus affected by *induced* electricity. If, instead of air, a plate of mica or glass be between the two conductors, the same phænomena will occur; so that it would appear that the conductor merely gains two opposite electricities, or polar electricities, of the same kind as those of the nonconductor. The phænomena of sparks, of discharges, and of accumulated electricity, depend upon this law. In the case of the common electrical spark, a stratum of air is charged in the same manner as a glass bottle, partially coated with tin foil, is charged in the Leyden experiment; when the hand is held near the positive conductor of an electrical machine, the person standing on the ground, the hand is rendered negative, and the states become exalted, till the polarities, as they may be called, are annihilated through the air, producing a spark, a snap, and a distinct sensation. If a number of small pith balls, placed upon a surface of metal, are caused to approach an electrified body, they are brought into the opposite state by induction, and are attracted towards the body; but when they come in contact with it, this state is destroyed, they gain the same state, and are repelled."

So far the phænomenon of electrical polarity is perfectly analogous to that of magnetism. The coincidence favours the reveries of speculators relating to an electrical and magnetic atmosphere. As to the term *induction*, which is more familiar to metaphysical than physical language, it seems as convenient and applicable as any other. Mr. G. Singer, indeed, in his lectures on electricity made some objections to it; but as he proposed to state his reasons in the Philosophical Magazine, we shall leave his opinions to his own enunciation. "In consequence of the principle of induction," says Sir H. "the condensing electrometer is much more sensible than the common electrometer. This instrument consists of two plates of polished metal, the surfaces of which are parallel, one connected with the plate of the electrometer, the other moveable, in connection with the ground, and the plates are very near each other. When the body supposed to be electrical, is made to touch the top of the electrometer, and is afterwards removed in separating the polished plates, the effect will be perceived."

"The difference in what are called the conducting powers of bodies, seems to depend entirely upon the dif-

 ferent

ferent manner in which they receive the electrical polarities; or in which their parts become capable of communicating attractive or repellent powers to other matter. Nonconductors appear to receive polarities, only with great difficulty, but retain them for a long while, and present probably a number of different alternations of poles, within a small space, and cannot be affected to any great distance. Imperfect conductors receive polarity with more facility, but present fewer alternations, and preserve their electricities for a shorter time. Perfect conductors are easily affected throughout; but present at most only two poles, and the powers rapidly destroy each other. The difficulty with which nonconductors receive polarity, is shown in the phænomena of charging thick and thin coated plates of glass and mica. The thin plates are capable of being charged much more highly than the thick ones, and the accumulation on the opposite surfaces is much greater. Rarefied air or gaseous matter, is much more susceptible of receiving polarities, than dense air or gaseous matter; and hence, the electrical spark will pass much further through rarefied air or light gases, than through dense air or heavy gases; it passes much further likewise in gases, than in nonconducting fluids."

The terms *quantity* and *intensity* of electricity are admitted to be rather vague, although useful; but if we consider the former in its natural sense of volume or measure, and the latter as indicating an increased impetus, we may form sufficiently definite ideas from these expressions. Thus, for instance, where the *quantity* of electricity in any conducting body is not altered, but the *intensity* is, the latter is always in proportion to the diminution of the discharging surface; hence the more the surface is diminished the more the electricity is rendered intense. In this respect, it may be said to be condensed or concentrated, and the more condensed, the greater its intensity. This is further confirmed by the author's observation, that "when very small conducting surfaces are used for conveying very large quantities of electricity, they become ignited; and of the different conductors that have been compared, charcoal is most easily heated by (Voltaic) electrical discharges, next iron, platina, gold, then copper, and lastly zinc. The phænomena of electrical ignition, whether taking place in gaseous fluid or solid bodies, always seem to be the result of violent exertion of the electrical attractive and repellent powers, which may be connected with motions of the particles of the substances affected." Here Sir H. adduces another

another argument or apparent fact before alluded to, to disprove the existence of a subtile fluid as matter of heat, that a "platina wire may be preserved in a state of intense ignition in vacuo, for an unlimited time; yet such a wire cannot be supposed to contain an inexhaustible quantity of subtile matter." Should it, however, afterwards be discovered that electric matter, and the matter of heat, (if there be any such thing) are identical substances, only perceived by us at present under different modifications, the explanation of these phænomena must be much more simple and consonant with all our other knowledge of physical bodies. Tourmaline, boracite, and many other minerals become electric by heat; in fact, there is no existing vitreous or resinous electricity without sensible heat. We find much less difference between the exterior influence of what is commonly denominated matter of heat, and the electric fluid, than between hydrogen, oxygen, and water. Analogy seems to sanction this: thus, most of the substances which act distinctly upon each other electrically, are likewise such as act chemically, when their particles have freedom of motion. It is the case with the metals, with sulphur and the metals, and with acid and alkaline substances. Metals having the highest attracting powers are positive; as for instance, zinc is positive with respect to iron, the latter to tin, tin to lead, lead to copper, copper to silver, silver to gold, gold to platina, and the latter to charcoal. In stating the hypothesis of "the possibility of the dependence of electrical and chemical action upon the same cause," the author complains of misrepresentation, as if he had maintained or supposed "that chemical changes were occasioned by electrical changes." On the contrary, he only supposed them "produced by the same power, acting in one case (electric effects) on masses, in the other case (chemical effects) on particles. The controversy, however, was excited with a view not less to notoriety than philosophical truth, or the development of the laws of matter. In all such cases silence is the best argument; and it is a serious injury to science to waste time on an artificial phantom, which might be employed in exploring new and unknown paths in the vast fields of philosophical research.

Among the multiplicity of facts here stated, should be noticed the experiment proving that the action of the common electrical machine does not depend on the oxidation of the amalgam. Sir H. found that a small machine properly mounted was active in hydrogen gas, and more active in carbonic acid gas than in the atmosphere, probably

 owing

owing to its greater density. This has been so often repeated as to be deemed quite conclusive. Before terminating these remarks on electric attraction and repulsion, the Professor very judiciously guards the young student against idle speculations respecting electricity and sensitive action. "The laws of dead and living nature," he truly observes, "appear to be perfectly distinct: material powers are made subservient to the purposes of life, and the elements of matter are newly arranged in living organs; but *they are merely the instruments of a superior principle.*" This sovereign truth deprives the modern materialists of the suffrage of the first chemist and philosopher of the age.

The section "on analysis and synthesis, the circumstances to be attended to in these operations, and the arrangement of undecompounded bodies," equally abounds in new and luminous views of chemical phænomena. One instance will suffice to show that the results of even the most familiar experiments have been strangely misinterpreted, and that almost the very basis of all our chemical theories consists of errors and misconceptions. Thus; "when concentrated oil of vitriol, which consists of sulphuric acid and water, is poured upon common salt, and they are heated together, muriatic acid gas flies off, and sulphate of soda is obtained: hence it was concluded, that common salt consists of muriatic acid gas and soda; and that the sulphuric acid merely displaced the muriatic acid gas; and no account was taken of the water of the sulphuric acid in the operation: yet the whole change depends upon this water; and no soda and no muriatic acid can be procured from common salt, without water; and common salt is made directly by heating sodium, the metal which I discovered to be the basis of soda, and chlorine together, and these are both as yet undecompounded bodies; and if 92 parts of oil of vitriol, which consists of 75 parts, by weight, of sulphuric acid, and 17 parts of water, be made to act upon 111 parts of common salt, which consists of 44 sodium, and 67 chlorine, the water will be decomposed, 15 of oxygen will combine with the sodium to form 59 of soda, and two of hydrogen will combine with 67 of chlorine to form 69 of muriatic acid gas, and the sulphate of soda will be 134 parts."

The second division, of radiant or ethereal matter, and its effects in producing vision, heat, and chemical changes, shows how much we are still deficient in correct data on these curious and important points. Sir H. inclines to the Newtonian theory of light, but seems to admit Rumford's ford's notion that it is not a specific fluid. The Bavarian Count however goes further, and compares light to sound; but Sir H. does not include *sound* in his radiant matter, and if light be matter at all, it cannot be considered as perfectly analogous to sound. There is also much reason to doubt the soundness of the Count's doctrine of light, as his lamp, which he presented to the Royal Society, has proved by experience totally inadequate to the purpose of emitting light. His series of burners contiguous to each other may possibly have illuminated his own imagination; but they have failed to enlighten the presiding room of the Royal Society.

Empyreal or undecompounded substances that support combustion constitute the author's third division. In considering their combinations with each other, Sir H. shows, that although "almost all cases of vivid chemical action are connected with the increase of temperature of the acting bodies, and a greater radiation of heat from them, and in a number of instances, light is also produced; yet no *peculiar* substance or form of matter is necessary for the effect, and that it is a *general* result of the actions of any substances possessed of strong chemical attractions or different electrical relations, which takes place in all cases where an intense and violent motion can be conceived to be communicated to the corpuscles of bodies." The strength of the attraction determines the rapidity of combination, and the latter the intensity of heat and light. This is a much more simple and more natural exposition of the actions of matter, than the supposed legerdemain operations of phlogiston or oxygen, which have been so confidently maintained by theorists. Many hitherto undecompounded bodies, which cannot easily be supposed to contain oxgyen, produce heat and light by their mutual chemical action; such are potassium in combining with arsenic and tellurium, and sulphur with certain metals become ignited by their union. Oxygen and chlorine or oxymuriatic gas are the substances that support combustion, and here considered as undecomposed or simple elements, in the present state of our knowledge.

In the fourth division, on undecompounded non-metallic inflammable or acidiferous substances, are hydrogen, azote, sulphur, phoshorus, carbon, and boracium or boron, all of which "are capable of combining with oxygen, and all except azote and charcoal with chlorine." The latter exception, such is the rapid march of the author's discoveries, is is no longer correct, as his recent discovery of the extraordinary detonating substance from the combination of chlorine with ammonia, renders it probable that chlorine may be combined with azote. Of the various acids denominated from nitre, Sir H. observes that according to the principles of the French nomenclature, they should be called *hydro*-nitric or *hydro*-nitrous. When the author discovered a solid hydrat of phosphorous acid in Feb. 1812, he also recognised the existence of an elastic fluid, produced with solid phosphoric acid by heating the hydrat in a retort out of the contact of air. This product he calls hydrophosphoric gas, which in specific gravity is twelve times heavier than hydrogen. It contains four proportions of hydrogen, and one of phosphorus; water absorbs about one-eighth its volume of it. Phosphoretted hydrogen probably contains this gas in many experiments. The basis of borax, which the Professor originally concluded to be metallic, and consequently denominated it boracium, he now calls *boron*, as subsequent experiments have not demonstrated its metallic nature. Boron is an opake, dark olive-coloured powder, infusible and not volatile at any known temperature; when strongly heated in contact with air it burns, and forms dry boracic acid. It is a non-conductor of electricity, and insoluble in water. Much still remains to be discovered respecting the operations of this substance and its application to the arts, owing perhaps to its hitherto comparative scarcity. Of the metals, Sir H. has augmented their number to 39, five times the number known to our ancestors. The sixth division, on substances the nature of which is not yet certainly known, treats of the fluoric principle, and the amalgam procured from ammoniacal compounds.

The seventh and last division of this part embraces a view of the analogies between undecompounded substances, ideas respecting their nature, the modes of separating them, and the relations of their compounds. Here the author's almost universal knowledge of physical or chemical bodies enables him to trace the most curious and interesting analogies in the appearances and operations of matter, and the laws of the material world. The chain of gradations in the metals, inflammable and other bodies, leads to the conclusion that, as far as our knowledge of the nature of compound bodies has extended, " analogy of properties is connected with analogy of composition. We know nothing of the true elements belonging to nature;

but

but as far as we can reason from the relations of the properties of matter, hydrogen is the substance which approaches nearest to what the elements may be supposed to be. It has energetic powers of combination, its parts are highly repulsive as to each other, and attractive of the particles of other matter. It enters into combination in a quantity very much smaller than any other substance, and in this respect it is approached by no known body." It seems that "in the electrization of a globule of mercury in water oxygen appears to be combined with the metal, and yet no hydrogen evolved." This experiment would sanction the belief that water is the ponderable basis of both oxygen and hydrogen; but more experiments and circumstances are wanting to allow of such an opinion being seriously entertained. The whole of this part should be attentively studied by all clergymen who wish to conscientiously discharge their duty; the sublimest conceptions of the Deity are augmented by an enlightened consciousness of our own inability to attain absolute demonstration and certainty, even in the laws of surrounding and sensible matter. The pious yet sublime opinions of a Newton and a Davy have done more good to Christianity, and consequently to the true interests of society, than all the religious controversies from the days of Arius to those of Priestley.

In an Appendix we find some interesting tables of the quantities of oxygen and metal which combine together in different states.

"M. Berzelius has had the goodness to communicate to me the following estimates, some of which agree very nearly with those given in the preceding pages; others are new, and all afford evidences of the truth of the theory of definite proportions. It it peculiarly satisfactory to me, to be able to state the coincidence of so many of the conclusions of this distinguished chemist with my own results, obtained usually by very different methods of operation.

"*Of the Oxides of Antimony.*

	Metal.	Oxygen.	Metal.	Oxygen.
First Oxide	100	4,65	96,826	3,174
Second	—	18,6	84,317	15,683
Third	—	27,9	78,19	21,81
Fourth	—	37,2	72,85	27,15

The Sulphuret of Antimony is composed of 100 parts of metal, and of 37,25 parts of sulphur.

"*Oxides*

"*Oxides of Tin.*

	Metal.	Oxygen.	Metal.	Oxygen.	
First Oxide	100	13,6	88,03	11,97	1
Second	—	20,4	83,13	16,87	$1\frac{1}{2}$
Third	—	27,2	78,61	21,39	2

The Sulphurets of Tin.

	Metal.	Oxygen.	Metal.	Oxygen.	
First	100	27,234	78,6	21,4	1
Second	—	40,851	71,8	28,2	$1\frac{1}{2}$
Third	—	54,468			2

The Oxide of Tellurium.

100 parts of metal, with 24,83 parts of oxygen.

Telluretted Hydrogen.

Tellurium 100 parts. Hydrogen 1,948.

The Oxides of Gold.

	Metal.	Oxygen.	Metal.	Oxygen.	
First	100	11,026	96,13	3,87	1
Second	—	12,077	89,225	10,775	3

The Oxides of Platinum.

	Metal.	Oxygen.	Metal.	Oxygen.	
First	100	8,287	92,35	7,65	1
Second	—	16,574	85,9	14,1	2

The Oxide of Palladium.

Metal 100. Oxygen 14,055.

Sulphuret of Palladium.

Metal 100. Sulphur 28,15.

The Oxides of Manganesum.

	Metal.	Oxygen.	Metal.	Oxygen.	
First	100	7,0266	93,435	6,565	1
Second	—	14,0533	87,68	12,32	2
Third	—	28,1077	78,1	21,9	4
Fourth	—	42,16	72,25	27,75	6
Fifth	—	56,215	64	36	8

Metallic Oxides examined by other Swedish Chemists.

Oxides of Mercury, by M. Sefstrom.

First,	Metal 100.	Oxygen	3,95	1
Second,	—— 100.	——	7,9	2

Oxide of Bismuth, by M. de Lagerhielm.

Metal 100. Oxygen 11,275.

"*Oxide*

"*Oxide of Nickel*, by M. de Rolhoff.

First, Metal 100.	Oxygen 27,3	}	1
Second, —— —.	—— 40,95	}	$1\frac{1}{2}$

Oxide of Cobalt, by the same.

First, Metal 100.	Oxygen 27,3	}	1
Second, —— —.	—— 40,95	}	$1\frac{1}{2}$

Oxide of Cerium, by M. de Hisinger.

First, Metal 100.	Oxygen 17,41	}	1
Second, —— —.	—— 26,115	}	$1\frac{1}{2}$"

The preceding extracts from these "Elements of Chemical Philosophy" sufficiently prove that this is the first truly scientific work on chemistry in our language. The superficial production of Fourcroy, although dignified with the title of "philosophy," is rather a concise systematic arrangement of chemical substances, and is incomparably inferior, not merely in original views, but in scientific elucidations and intellectual precision. The present volume gives us some reason to hope that, before finishing his work, the author will be able to furnish the public with a new and improved chemical nomenclature, better adapted to truth, nature, and the genius of our language, than that imported from France. It is quite time that we should consider the English as a language and not a dialect, and cease to let it be influenced by every vague fancy of the French. Even the very name of this science has fluctuated in humble imitation, and we have now almost universally Chemistry for Chymistry, according to etymological propriety. We regret that Sir H. has admitted the barbarous word *potassa* into his pages. It is a most useless and absurd combination of letters adopted by Murray under the idle pretext of scientific distinction; its origin however tends to show the thoughtless promptitude with which we have hitherto transferred French words into English. Of the English term potash the French made *potasse*, and Mr. Murray made it again into *potassa*, pretending to avoid its being confounded with the potash of commerce. Commercial men however say "Dantzic ashes," or "American potash," so that there is no danger of the impure substance in trade being confounded with the potash of chemists. But, besides the literal barbarism of *potassa*, it has other and more serious inconveniences; it encumbers chymical science with a superfluous term, augments the already too copious nomenclature, and embarrasses the memory of the young student, without conveying any new idea, or being

of

of any utility whatever. The author's papers in the Philosophical Transactions are hitherto free from this affectation, and we hope he will see the propriety of using only pure potash in future in these Elements. Sir Humphry is no less distinguished for the sublimity and beauty of his language than for his unparalleled discoveries: consequently his example in such a case must naturally become the *jus et norma loquendi.*

We understand that translations of this volume into Portuguese and Spanish have already been undertaken by gentlemen very well qualified for the arduous task.

LXXVIII. *An Account of a Series of Observations made on the Distance of the Sun and Moon for determining the Longitude of the Place of Observation, with Remarks on the best Method of making such Observations, with their Calculation, Deductions, &c. By Mr.* Firminger, *late Assistant at the Royal Observatory, Greenwich.*

It being generally understood, that to determine the longitude of a place by the lunar observations, it is requisite to have three separate observers, viz. one to observe the moon's altitude, another the sun's, and a third to take the distance between the limbs of the sun and moon, or the moon and a star, I was induced to make a set of observations for determining the longitude of my place without any such assistance, it having frequently occurred to me that a series of observations might be made by only one observer, which would require but little additional labour in the calculation to that which is necessary when three separate observers are employed, and the result of the trial has fully confirmed my conjecture.

The following observations, from whence this conclusion was drawn, have not the best possible arrangement; it was readily seen in making the calculation, that much of the trouble of reduction might have been avoided by a different method of taking the observations; but what has been done will, it is hoped, be sufficient to show the value of the means which have been employed.

The weather at the time of the year when these observations were made, is but seldom favourable in London, and it was owing chiefly to this circumstance that different sets of observations on the same day were taken, and which it was intended to repeat till the number of independent sets amounted to ten, by taking two or three sets each day; but the fine weather

weather did not continue sufficiently long for this purpose; nor has an opportunity offered since the period at which these observations were made to repeat them. The sets were four in number; two were taken on the 25th of October, and the remaining two on the 27th. The sun's and moon's altitude were taken from an artificial horizon, and the time at each observation noticed from a common horizontal watch showing seconds: great accuracy was not regarded in taking the moon's altitude, nor indeed of the sun's, except that set from whence the time was to be computed, for a small error in the observed altitude cannot materially affect the correction to be applied in reducing the apparent to the true distance: great care however should be observed in noticing the moment when the distance is completed, as, whatever error is committed here, will appear in the longitude, that being deduced by a comparison of the time at Greenwich, given by the observed distance compared with the distance deduced from the Nautical Almanac, and the time inferred from the sun's observed altitude.

The method which I should advise would be, first, to take a single observation of the altitude of the moon's enlightened limb, then one of the sun's lower limb*, and when these have been completed, take with the greatest care five distances at least of the proper limbs of the sun and moon. When these are finished, take another altitude of the sun, then one of the moon; these four altitudes of the sun and moon will be sufficient for clearing the distance; and if they are taken at nearly equal intervals of time before and after the time at distance, it will be only necessary to take a mean of each two corresponding altitudes, which will very nearly correspond to the apparent altitudes when the distance was taken; but a set of observations of the altitude of the proper limb of the sun should now be taken, and the time noted down with the greatest care. These observations should not be less than five, because from them the apparent time at distance is to be inferred by a comparison with the watch. From this method of proceeding little difficulty occurs, or additional trouble to the usual method by three separate observers; and I am induced to think that it possesses many advantages over that method in point of accuracy, for in the latter case the whole is com-

* This applies equally to a star when that is used instead of the sun, and is to be so understood in all cases here referred to.

pleted

pleted by one observer; and if he has a good eye, and is careful in making his different sets of observations, it is more than probable that the whole will be conducted with much greater accuracy than when three separate observers are employed, as it seldom happens that each of the observers is equally correct and careful; and should it be discovered that the one who takes the sun's altitude is inaccurate, the whole deduction will be affected with that inaccuracy: nor indeed is it scarcely possible for each observer taking the altitudes to complete his observation at the same time that the one who takes the distance has completed his; and the inaccuracy arising will be of considerable importance, should this happen to the observer who observes the object from whence the time is to be computed.

In the method I have adopted, the only inaccuracy affecting the observation arises from the altitudes of the bodies not increasing or decreasing uniformly: this, in the sun or a star, is of no importance if the observer is at all expert at making his observation, and in the moon the error will be too small generally to deserve notice. The watch by which the observations are made, should have a seconds hand, and ought not to vary more than one minute in twenty-four hours;—a good horizontal or duplex watch will answer very well, and will allow time for the observer to take his final set of observations for determining the error of the watch without hurrying himself.

The deductions derived from a mean of the four sets of observations about to be enumerated, give the longitude of the place in time 38,9 sec. west of Greenwich; or in space 9′ 31″,35 west*. By a reference to the best maps of London, it appears that the Polygon, Somers Town, is about 400 yards to the east of St. James's Church, Piccadilly, and the longitude of this church is according to the trigonometrical survey, 8′ 5″ west of Greenwich. The observations give the longitude of the Polygon, Somers Town, 1′ 26″,35 to the west of St. James's Church, or about one mile; this added to the distance that the Polygon is to the east of St. James's Church, makes the whole error in longitude deduced from these observations about one mile and a quarter, which, taking into consideration the errors of observation, of the instruments, and of the lunar tables, will be found very small; and I am fully persuaded

* The observations were taken near the Polygon.

that

that had the number of observations been augmented to ten, and a mean of the results taken, a great part of the present error would have been removed.

I shall conclude these deductions by remarking, that a complete set of observations similar to those I have here given may be taken in the space of a quarter of an hour, or twenty minutes, by a single observer, and that they may be facilitated, and rendered a little more accurate, by the assistance of some person to write down the observation at the time: this part of the business, indeed, any one may do, and I am inclined to believe that under such circumstances the greatest error committed, will seldom exceed five miles, but will generally fall short of this distance.

I have reduced the apparent distance to the true by the method given in the supplement to the 3d edition of the requisite Tables, a method both simple and expeditious, and so easy in practice that a child of eleven years of age of moderate capacity might in a short time be fully instructed to perform the calculation.

I cannot help remarking, that many attempts have been made, and are still making, to construct machines for the more easily determining the longitude at sea; and although I am far from wishing to discourage any such attempts when conducted upon scientific principles, yet I am greatly surprised that the means of determining the longitude by the lunar observations should even at this time be thought an operose task. The improvements in the lunar tables, in the construction of sextants, and in the method of reducing the apparent to the true distance, have been such as to ensure the observer, with even but a moderate degree of experience, an accuracy far greater than is ever required in nautical practice. It may be said, that the weather is oftentimes too unfavourable to determine the longitude by the lunar method, and that for several days in the month the moon is invisible, being too near the sun. Here the use of a good time-keeper would be of great advantage, as by it the longitude would be easily carried on between the opportunities which offer, to obtain distances of the sun and moon, or the moon and a star, and even a time-keeper of very moderate performance would answer this purpose with sufficient accuracy for every nautical purpose: would it not therefore be of great advantage to the maritime department of this country to have young gentlemen designed for the service of the sea, well educated on shore, both in the practice as well as theory of finding the longitude by the means here recommended?

October

October 25, 1812.—The morning being very fine, the following observations of distances and altitudes were taken for finding the longitude:

N.B. 27″ is to be added to all the observations to the 27th inclusive.

H.	M.	S.					
22	41	3	Time by watch.	42° 36′ 0″	Double alt. ☽'s U. L.
22	43	5	Ditto	97 49 10	Dist. nearest limbs, ☉ & ☽
22	47	4	Ditto	48 55 50	Double altitude ☉'s L.L.
22	57	23	Time by watch	37′ 38″ 40	Double alt. ☽'s U. L.
23	2	0	Ditto 2nd set	97 40 00	Dist. nearest limbs ☉ & ☽
23	5	3		50 14 00	Double alt. ☉'s L. L.

	H. M. S.		
Oct. 27th.	20 44 00	94 11 40	Double alt. ☽'s U. L.
	20 48 00	74 53 11	Dist. nearest limbs ☉ & ☽
	50 20	51 50	
	52 50	51 10	
Mean	20 50 23	74 52 37	
	20 55 00	31 5 30	Double alt. ☉'s L. L.
	55 49	15 40	
	56 40	25 20	
Mean	20 55 49·6	31 15 30	
	21 0 8	90 55 30	Double alt. ☽'s U. L.
	1 7	35 40	
	2 3	20 30	
Mean	21 1 6	90 37 13·3	
	21 5 31	33 14 40	☉'s L. L. double alt.
	7 6	33 10	
	7 44	41 20	
Mean	21 6 47	33 29 43	
Second set	21 16 2	87 11 10	Double alt. ☽'s U. L.
	21 19 23	74 41 10	Dist. nearest limbs ☉ & ☽
	20 16	41 00	
	21 21	40 40	
Mean	21 20 20	74 40 56·6	
	21 53 31	36 44 50	☉ L. L. double alt.
	24 14	52 20	
	24 55	37 0 10	
	21 24 13·3	36 52 26	
	21 26 38	84 46 20	Double alt. ☽'s U. L.

Calculation

Calculation of the observations of the distances of the Moon's remote limb from the Sun, taken on October 25th and following days.

Calculation of the First Set, taken October 25th, 1812.

☽'s H Par. N. alm. 57′ 19″ } 1′ 55″ } 52′ 59″ Tab. ix. 9.997589
Appt. alt. ☉'s ct. 24 44 4 } 51 4 }
Ditto ☽'s do. 21 2 22 x. — 08

	3 41 42	N. V.	002079		9 997581
Dif. appt. alts.	98 21 22	N. V.	1.145326		
			1.143247	Log.	6.058140
			1.136896	Log.	6.055721
Dif. true alts.	2 48 43	N. V.	0.001204		
True distance	97 56 16	N. V.	1.138100		

☉'s L. L.	48° 58′ 15″	Double alt.	
	24 28 7		
	— 2 7		
	+ 8		
	+ 16 8		
True alt.	24 42 16	☉'s centre.	
☉'s P. D.	102 29 7	cosec.	0.0103938
Latitude	51 31 46	sec.	0.2061312
Sum	178 43 9		
Half sum	89 21 34	cos.	8.0484251
Remainder	64 39 18	sin.	9.9560467
		2]	18 2209968
	7° 24′ 36″	s	9.1104984
	8		
	0 59 16 48		
	24		
	23 0 43 12		
	— 3 59	Reduction to dist.	
	22 56 44	Appt. Time.	

Distance per N. A. at		21ʰ	98° 55′ 29″
True distance			97 56 16
Distance per N. A. at		0	97 24 39
	2970	P. L.	1 30 50
	4828	P. L.	0 59 13
			H. M. S.
	1858	P. L.	1 57 21
			21
Time at Greenwich			22 57 21
Appt. time at place			22 56 44 of Observation.
		Longitude	37 W. fr. Greenwich.

Computation of the Second Set, taken October 25th.

☽'s h. par.	57′ 19″	1′ 51″	53′ 22″	Tab. ix. 9.997880
Appt. alt. ☽'s ct.	18 33 42	51 31		x. 08
Ditto ⊙'s ct.	25 23 8			
Diff. appt. alt.	6 49 25	N. V.	007083	9.997872
Appt. dist.	98 12 12	N. V.	1.142687	6.055228
			1.135604	6 053100
			1.130050	
Diff. true alts.	5 56 3		005358	
True dist.	97 46 56		1.135408	

2] 50° 14′ 27″	Double alt. ⊙'s L. L.	
25 7 14		
− 2 2		
+ 8		
+ 16 8		
25 21 28		
102 29 12	cosec.	0.0103938
51 31 46	sec.	0.2061312
2] 179 22 26		
89 41 13	cos.	7.7374966
64 19 45	sine	9.9548682
	2]	17.9088898
5° 9′ 57″	s	8.9544449
8		
0 41 19 36		
23 18 40 24		
− 3 3	Reduction to time at dist.	
23 15 37,4	Appt. time at Observation.	

October 25th, at 21h	98 55 29	Per N. Almanac dist.	
Ditto at Obs.	97 46 56		
Ditto at 0h.	97 24 39	P. L.	2970
	1 30 50	P. L.	4193
	1 8 33	P. L.	1223
	2 15 50		
	H. M. S.		
	2 15 50		
	21		
	23 15 50		
	23 15 37,4		

Longitude 6 W. from Greenwich.

Computation of the First Set, observed October 27, 1812.

☽'s H. par.	55′ 47″	3′ 15″	40′ 58″		Tab. ix.	9.995103
☽'s appt. alt.	46 10 47	37 43			Tab. x.	−7
☉'s appt. alt.	15 31 47					
	30 39 0		N. V.	139702		9.995096
Appt. dist.	75 23 51		N. V.	747888		
				608186		5.784536
				601357		5.779132
Dif. true alt.	31 19 58			145838		
True dist.	75 21 23			747195		

☉'s alt. observed	2] 31 15 57		
	15 37 58		
	+ 16 7.8		
	+ 8.0		
	− 3 23		
☉'s true alt.	15 50 51		
☉'s P. D.	103 8 4	cosec.	0.0115126
Latitude	51 31 46	sec.	0.2061812
Sum	170 30 41		
Half sum	85 15 20	c	8.9175663
Remainder	69 24 29	s	9.9713264
		2]	19.1065365
	20 56 47	s	9.5532683
	24^h 8		
	2 47 34 16		
	21 12 25 44		
	− 5 26	Reduction to time at distance	
	21 6 59 7	Appt. time at distance	

October 27, at 21H	75° 25′ 2″	Dist. per N. Almanac	
At Obser.	75 21 23	Ditto	
28th noon	73 59 35	Ditto	
	1 25 27	P. L.	3236
	3 39	P. L.	1.6930
	7′ 42	P. L.	1.3694
	9h.		
	9 7 42	Time at Greenwich	
	9 7 0	Appt. time at place of observation	
Longitude	42	W. from Greenwich.	

Reduction of the Second Set of Observations taken on Oct. 27, 1812.

```
☽'s H. Par.      55′ 46″ ⎫ 39′ 41″ ⎫
Appt. alt. ☽  43  6  16 ⎬  2  44  ⎬ 42 25                  9·995372
Do.        ⊙  38 11  46 ⎭         ⎭                         −     07
              24 54  30  N. V.  093016                      9·995365
              75 12  44  N. V.  744760
                                651744                      5·814078
                                644827                      5·809443
              25 36  55         098283
True dist.    75° 6′ 52″        743110

Double Alt. ⊙'s LL 2) 36 52 54
                      18 26 27
                    + 16  8
                    −  2 49
                       + 8
⊙'s True Alt.         18 39 54
⊙'s P. D.            103  8  4  cosec. 0·0115126
Latitude              51 31 46     sec 0·2061312
                  2) 173 19 44       c 8·7648002
                      86 39 52       s 9·9671642
                      67 59 58   2) 18·9496082
                      17 21 42       s 9·4748041
                              8
                  2h 18 53 36
                  24
                  21 41  6 24
                   − 3 53      reduction to time at distance.
Appt. Time        21 37 13  4  at distance

Distance at       21H. 75° 25′  2″  per Nautical Almanac.
Obser.                 75   6 52
28th noon              73  59 38
                        1  25 27  PL 3236
                        0  18 10  PL 9960
                        0h 38 17  PL 6724
                       21
Time at Greenwich      21 38 17
Appt. Time at Place    21 37 13
Longitude from Greenwich   1  4  W.
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By

By collecting together the different results given above, they will stand as follows:

			M.	S.	
Oct. 25th.	1st Set	Obs.	0	37	W.
	2	Do.	0	12,6	W.
Oct. 27th.	1st Set	Do.	0	42	W.
	2	Do.	1	4	W.
		4)	2	35,6	
Mean from Greenwich				38,9	W.

Hence the Longitude derived from the above observations comes out 38,9 s. in time West of Greenwich, or equal to 9′ 31,35″ in space. The reason for giving the calculations at full length has been merely to show their simplicity; and should they tend to induce other persons interested in the determination of this useful problem to repeat them, the author's intentions will be answered. His object has been in this instance directed only to an endeavour to facilitate the acquirement of the method of determining the Longitude by a means both simple and certain.

Somers Town, Dec. 27, 1812.

LXXIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Nov. 26. The Croonian Lecture, by Dr. Wollaston, was read. It was an unusually brief lecture, and treated only on the integral molecules of crystals. Dr. W., improving an ingenious speculation of Dr. Hooke, assumed the principle, that the original molecules of matter are globular; and with these globes, he very satisfactorily showed how many of the primitive forms of crystals might arise from their disposition. He acknowledged that the assumption was perfectly gratuitous; but alleged, that at present there is no other mode of accounting for the tetrahedral and other forms of crystals.

Saturday the 30th November, being St. Andrew's day, the Royal Society held their annual meeting, at their apartments in Somerset Place, for the choice of a Council and Officers for the year ensuing, when the following gentlemen were elected:

Of the old Council:—The right hon. Sir Joseph Banks, Bart. K. B.; Sir Charles Blagden, Knt.; Samuel Goodenough, Lord Bishop of Carlisle; Anthony Carlisle, Esq.; Sir Humphry Davy, Knt.; Samuel Lysons, Esq.; Joseph de de Mendoza Rios, Esq.; George Earl of Morton; John Pond, Esq., astronomer royal; William Hyde Wollaston, M. D.; and Thomas Young, M. D.

Of the new Council:—George Earl of Aberdeen; Taylor Combe, Esq.; Sir Thomas Frankland, Bart.; Silvester Lord Glenbervie; Philip Earl of Hardwicke; Matthew Raper, Esq.; Samuel Rogers, Esq.; Smithson Tennant, Esq.; Rev. William Tooke; and Roger Wilbraham, Esq.

Officers.—The right hon. Sir Joseph Banks, Bart. K. B., President; Samuel Lysons, Esq. Treasurer; William Hyde Wollaston, M.D. and Taylor Combe, Esq. Secretaries.

Dec. 10. The Society again assembled, the right hon. Sir Joseph Banks, Bart. President, in the chair, when a paper by Mr. Smithson on ulmin was read. Mr. S. was so fortunate as to procure a specimen from the same gentleman at Palermo, who originally forwarded it to Klaproth, and consequently operated on a substance perfectly similar to that of its original discoverer. The author found that the characters given to the substance called ulmin by Dr. T. Thomson are very incorrect; and instead of its being a resinous substance, he found that it chiefly consisted of extractive matter united with potash. It was very slightly soluble in alcohol. Mr. S. made some experiments on the juice and branches of English elm, which he found somewhat different from the substance sent him from Sicily. He did not, however, pursue his experiments so far as to determine the exact nature of the matter obtained from the native tree.

Dec. 17. Dr. Wollaston, availing himself of the facts reduced to practice by Leslie, in freezing water by means of evaporation in vacuo, described an instrument with which this process might be effected for amusement. Taking a glass tube of any length, such as used for barometers, making a bulb on each end of it four times its diameter, nearly filling one of these bulbs with water, exhausting the tube of air by boiling the water, and hermetically sealing it like a thermometer, and afterwards plunging the bulb into a mixture of salt and snow, ice would be immediately formed. This might be repeated as often as wished, in a tube three feet long, when the effects of extracting heat from the water would be very distinct to the eye.

The astronomer royal, Mr. Pond, laid before the Society a table of the latitudes of some stars near the north pole, ascertained with great accuracy by means of the newly-erected mural circle, made by Mr. Troughton, and which Mr. P. seems to consider as approaching perfection.

Sir

Sir Everard Home furnished a short paper, containing some remarks on the solvent glands and gizzards of the different species of cassowary in New South Wales, and the African ostrich. It appears that these organs are always adapted to the native climate and to the quantity of food which the fowl can procure. The length of the intestines, and their magnitude, seem dependent on the like cause.

A paper by Mr. Brande, on alcohol, as supplementary to his former remarks on this subject, was read. It appears that Mr. B. has at length succeeded in procuring alcohol from various fermented liquors, without the aid of distillation. For this purpose he operated with acetat of lead and nitrat of tin, by which he obtained pure alcohol. He also corrected several of his former estimates of the quantity of alcohol in wine, &c.

Dec. 24. Part of a second paper, by Dr. Lambe, on arsenic, was read. In the account of his experiments Dr. L. was led to suspect that carbon, azote, and hydrogen, are only modifications of the same element. The Society then adjourned till Thursday, Jan. 14, 1813.

*** In our last Number, where we reported the paper of the astronomer royal, which was read before the Royal Society on the 12th November, upon the last summer solstice, the mural quadrant was inadvertently mentioned: it should have been stated, that the observations from which the solstice was deduced, were made with the new mural circle lately erected at Greenwich.

GEOLOGICAL SOCIETY.

At a meeting of this Society on December 4th (the President in the chair) the reading of a paper by Wm. Philips, Esq. M. G. S. "on the veins of Cornwall" was begun.

The regular or metalliferous veins of Cornwall are found, with few exceptions, to run east and west. The known length of many of these veins is considerable, amounting in some instances to two or more miles, but their actual termination at either extremity has in no case been satisfactorily ascertained; all that is known being, that they gradually become so poor and narrow as to make it no longer worth the miner's while to pursue them. The dip or descent of the veins varies more or less from perpendicular inclining towards the north or south, which inclination is called the *underlie* of the lode. The depth of the veins is still less known than their longitudinal extent, not an instance having occurred of a vein being fairly worked out: many mines have indeed been relinquished, but only on account of the expenses of working them exceeding the produce. The deepest mine now in work in Cornwall is Dolcoath, some of the workings of which are 228 fathoms below the

 surface.

surface. The usual width of the veins that are worked varies from one foot to three; in particular instances, however, portions of veins occur 24 feet or even 30 feet wide, and on the other hand a vein of tin ore not three inches wide has been followed with profit.

The substances that accompany the metallic ores (or the veinstones) vary considerably, not only in different veins, but in different parts of the same vein; and it is from these, and not from their metallic contents, that the miner's nomenclature of the veins is derived.

Gossan is a friable substance of a loose texture, consisting of clay, mixed more or less with siliceous matter, and coated or tinged with oxide of iron; its colour varies from light yellow to deep red and brownish black. A gossany lode is more common than any other, and is considered as promising both for copper and tin.

When quartz predominates, the vein is said to be sparry; and if the quartz is considerably compact, it is looked upon as a very unfavourable indication, more especially if the vein becomes narrower as it descends.

If iron pyrites abounds, the vein is said to be mundicky. When this substance occurs at a shallower level, it is considered as not unpromising, more especially if mingled with yellow copper ore as it descends.

A vein containing a large proportion of chlorite is termed a *peachy* lode, and promises for tin rather than for copper.

A vein is said to be *flookany*, when one or both of its sides is lined with blueish white clay. It sometimes is so abundant as to occasion considerable difficulty and expense to prevent it from slipping down and obstructing the works.

When the contents of a vein consist of a hard substance of a greenish or brownish colour, which appears to be chiefly a mixture of quartz and chlorite, the vein is denominated *caply*. Tin is often found in it, copper rarely.

When the ore, whether of tin or copper, is found in detached stones or lumps, mixed loosely with the other contents of the veins, it is termed a *pryany* lode.

A vein abounding in blende is called a *black jack* lode, and is considered as unpromising for tin, but a good sign of copper.

When a vein contains granite in masses or blocks, or in a state of semi decomposition, it is termed a *growan* lode, and is generally considered as more promising for tin than for copper. Of late, however, many rich veins of copper have been found in the granite district of Cornwall.

The

The experienced miner by no means implicitly relies on even the most promising symptoms, for all of them at times are found to mislead; the following, however, are those in favour of which he is more especially prepossessed: all gossany lodes in general, the early discovery of pyrites with portions of yellow copper ore, also of blende and of galena, and the cutting of a good course of water, especially if it be warm.

The discovery of veins is effected in various ways: the ancient mode of *shoding* or tracing up water courses, when pieces of ore are found to occur among the rolled stones in their channels, is now rarely resorted to. The common method is to work drifts across the country from north to south, by which all veins in the district thus examined are sure to be cut through. Veins are often found in driving adits and levels for the working of known lodes: and not unfrequently are stumbled on by mere accident in digging ditches and foundations of walls.

December 18—The President in the chair—the continuation of Mr. Philips's paper on the veins of Cornwall was read.

The contents of a vein may be divided into those which are valuable and those which are not so; the latter, forming generally by far the largest portion, are technically called *deads*, and are left in the vein both to avoid the unnecessary expense of raising them to the surface, and for the very important purpose of preventing the two walls of the vein from collapsing, and thus destroying the works: in addition to the deads, strong pieces of timber are frequently made use of. Sometimes large wedge-shaped fragments of rock, called by the miners *horses*, occur in the vein, partially cutting off the regular contents of the lode, though seldom, if ever, entirely obstructing it. Veins of copper ore are however particularly liable to capricious and total obstructions without any obvious cause. In proportion as the rock becomes harder, the vein always becomes more narrow.

One of the first objects in opening a new mine is to drive an *adit* or horizontal gallery from the lowest convenient level, for the purpose of carrying off all the top water. One adit often serves two or three mines: and there is one (called the deep adit) which opens on one of the creeks of Falmouth harbour, the entire subterranean length of which is about 24 miles.

Copper veins, which fifty years ago were considered by the Cornish miners to be peculiar to schist, have of late beed found in the parishes of Gwennah and Redruth, to pass

pass freely from schist into granite, and back again to schist, without any deterioration. The texture and hardness of both rocks is liable to considerable variation, affecting of course the profit and progress of the miner often in a very remarkable degree. Two shafts of Huel Alfred were sunk in schist, and the cost of the one did not exceed *5l.* per fathom, while that of the other amounted to *55l.* for the same length.

The metalliferous or east and west veins are crossed by others, the direction of which is nearly north and south. These latter are called *cross courses*, and rarely produce copper or tin, or any other metallic substances. The principal practical advantage derived from these veins, especially when consisting of clay, is, that they oppose an effectual obstacle to the passage of water, and therefore the miners do not willingly pierce them without some adequate object in view. The disadvantage of them is, that they not only interrupt the course of the metalliferous veins, heaving them from a few inches to several fathoms, but not unfrequently totally impoverish them, so that a long and costly search after the heaved part of a vein often terminates in the mortifying discovery that it is not worth pursuing, as was strikingly exemplified in the corresponding veins of Huel Jewel and Tol Carn.

There is another species of vein called a contre or caunter, the direction of which is for the most part N. E. and S. W.—These are mostly if not always metalliferous, and often remarkably rich; of which the mines of Huel Alfred and Herland have afforded most splendid instances.

PHILOSOPHICAL SOCIETY OF LONDON.

The attention of the Society during this month has been directed to the subject of Hearing, by a Lecture on the Anatomy and Physiology of the Organ of Hearing in Man and other Animals, by Mr. T. J. Pettigrew.

The lecture commenced by observing the difficulty of treating anatomical and physiological subjects in a sufficiently popular manner to interest a general audience, and by professing an endeavour to divest it of technical phrases as much as possible.

The lecturer was aware that in many instances, in so complicated an organ as the ear, it was utterly impossible to avoid the use of the technical terms but in those he endeavoured to explain. For the difficulty was exceedingly great, when we consider that they are derived not only from

from the Hebrew, Arabic, Greek, Latin, French, Italian, Spanish, German, and English languages, but even from the Indian, African, and Mexican, as Mr. J. M. Good has shown, in a paper on Medical Technology, in the first part of a new series of the Transactions of the Medical Society of London. Notwithstanding the many difficulties which thus presented themselves in the demonstration, Mr. P., conscious of the utility of publicly disseminating so important a branch of knowledge, was emboldened to proceed to his subject "with all its imperfections on its head."

The ear he divided, in common with anatomists, into external and internal; the former, comprehending the lobulus pinna and meatus auditorius externus; the latter, the tympanum, labyrinth, and meatus internus. These several parts, together with the Eustachian tube and the auditory nerves, were perspicuously demonstrated by diagrams and preparations, and rendered intelligible to the audience. After this, Mr. P. proceeded to consider sound and its effects, and to apply them to the phænomena of hearing. He traced the sonorous undulations passing through the air collected into the concha, and conveyed along the canalis auditorius to the membrana tympani, where he noticed the discovery of a radiated muscle by Sir Everard Home, first in the elephant and afterwards in man, by which the membrane is brought into a state capable of acting, and of giving those different degrees of tension which empower it to correspond with the variety of external tremors: when the membrane is relaxed, the radiated muscle cannot act with any effect, and external tremors make less accurate impressions*. From the membrane he traced the progress of sound along the ossicula auditus, and considered the muscles belonging to them. From the last of the bones (the stapes) he traced it through the fenestra ovalis into the vestibulum and semicircular canals, and treated of the membranes lining those cavities, the fluid contained in them, and the nerves expanded upon the membranes. The cochlea, spiral laminæ, &c. then met with consideration, which parts Mr. P. conceived were subservient to the diversity of sounds; and he concluded the human demonstration by some appropriate observations on the Eustachian tube.

The concluding part of the lecture consisted of comparative anatomical observations. All vertebral animals (*i. e.* red-blooded animals) have an organ of hearing; it

* Croonian Lecture. Philosophical Transactions, part i. 1800.

is

is likewise found in many insects, and in some of the mollusca.

An abstract of this part we shall thus arrange according to the classes adopted by Linnæus:

Mammalia. Form of the ear various. In the porcupine it resembles the human. The cartilage composing the pinna more delicate than in man. The meatus auditorius externus, in animals whose nature leads them to dive or burrow in the ground, furnished with a valve which can be opened or shut at pleasure. Ex. Water shrew and mole.

Tympanum in all; membrane belonging to it, and an Eustachian tube entering it, passing from the fauces, except in the Cetacea, where it opens into the blowing hole.

Mastoid cells, or some structure approaching to it, in most; cellular in the pig, divided by bony septa; a mere cavity in the dog, cat, and others.

Ossicula auditus, generally four in number, in ornithorynchus paradoxus, only two.

Labyrinth in all quadrupeds. Cochlea, sometimes 3½ turns. Separate in the whales. Semicircular canals small.

Aves. No external ear; feathers peculiarly disposed around the meatus, which is valvular.

Tympana communicate with each other by cellular structure *.

Membrane convex externally. Ossiculum auditus single, connecting the membrane with the vestibulum.

Labyrinth. No cochlea; hollow bony process of similar conformation; semicircular canals projecting from the skull.

Amphibia. Crocodile the only one having an external meatus. [Of this the lecturer produced a beautiful preparation.] Many possess tympanum, Eustachian tube, and semicircular canals, as the turtle, in whom the membrane is cartilaginous. A single ossiculum. Frogs two ossicula. Concretions in the vestibulum.

Pisces. Internal ear grows proportionately to the size of the animal. Semicircular canals very conspicuous. Concretions of a very brittle nature in the vestibulum.

Insecta. Power of hearing doubted by many anatomists, but proved by Minasi, Scarpa, Comparetti, and others. Vestibulum discernible in the Cancer genus. Same nerves as supply the antennæ.

* The Elephant has a similar structure.

Vermes.

Vermes. Only distinct instance the Sepia officinalis. Vestibulum with concretions.

The lecturer concluded by observing, that throughout the animal kingdom, wherever there appeared an organ of hearing, a vestibulum containing a fluid, and surrounded by membranes upon which the nerves are distributed, was conspicuous; this, therefore, he conceived to be the most important part for executing the function of hearing. In the cuttle-fish it is a mere capsule; ascending in the animal kingdom, other parts are superadded till it reaches to man, in whom the organ is most complicated and most perfect; as he can not only distinguish simple vocal modulations and articulations, but also the different qualities with relation to one sound, and that too with wonderful accuracy*.

After the lecture, the learned president (Dr. Lettsom) adjourned the Society until the 7th of January, with a very neat and appropriate speech.

KIRWANIAN SOCIETY OF DUBLIN.

Dec. 16. A Memoir on the Malic Acid was read by M. Donovan, Esq.

After making some observations on the combinations of malic acid with lead, the author proceeded to notice certain defects in the processes hitherto proposed for preparing that acid. There is in the juice of apples a substance which has heretofore escaped observation, and which possesses very peculiar properties. In the process of Scheele, both are found in what has been supposed to be the resulting pure malic acid; nor can they be separated without the utmost difficulty. In the second process wherein nitrous acid is distilled from sugar, the author observed that a fluid is produced which does not always manifest in a decisive manner the characteristic properties of malic acid.

The author then adverted to some other defects of these modes of preparing the substance in question; and remarked, that even the process of Vauquelin, although by much the best, is not free from some of these objections. He then proposed an improvement of the latter process, which affords, as far as is at present known, a malic acid perfectly pure.

The paper concluded with conjectures concerning the state in which this acid, and perhaps vegetable acids in general, may previously have existed in fruits and berries.

* See *Cuvier, Leçons d'Anatomie comparée,* tome ii.

Some

Some experiments and observations were stated, which were supposed to render it probable that the *bitter principle* is a compound basis, which by uniting to oxygen, or by undergoing more complicated processes, might change its nature so far as to become an acid.

IMPERIAL INSTITUTE OF FRANCE.

[Continued from p. 394.]

Mineralogy and Geology.

The late professor Abilgaard, of Copenhagen, discovered some years ago a combination of alumine and fluoric acid, hitherto unknown to mineralogists. M. Brunn Neergardt, of the King of Denmark's household, published a historical notice of this very rare substance, which came from Greenland: he describes pieces in which it is surrounded by other minerals, a circumstance which may lead to the discovery of the kind of soil in which it is found.

M. Lelievre, member of the class, has published another notice of a gray mineral which he discovered in some pieces of granitic rock sent to him from Piedmont by M. Muthuon, mining engineer.

M. Brongniart has completed the mineralogical description of the environs of Paris, which he undertook in concert with M. Cuvier, by taking the level of the principal eminences in the district which he describes. The results of their joint labours have been given to the world, and will form a valuable addition to M. Cuvier's researches on fossil bones.

M. Dauxier Lavaysse, formerly a colonist of Saint Lucia, has presented to the class a geological description of Trinidad, and other islands adjoining the mouth of the Oronooko. The latter are very low, and are frequently inundated by the river, of which they seem to be mere alluvia. In Trinidad there is a lake which produces abundance of bitumen, and towards the southern shore the sea also throws up the same substance at two places. Two adjacent hillocks have small craters, and give out sulphureous vapours. We there find sulphur, alum, and crystallized vitriol. In another part of the island there is a mine of plumbago and coal. To conclude, Trinidad resembles in such a striking manner the adjoining continent in the nature of its rocks, that there is every reason to believe, according to M. Lavaysse, that it was anciently part of the same continent. Gray schistus or argill is every where prevalent: calcareous stone and gypsum, so abundant in the Antilles, are here very rare.

Vegetable

Vegetable and Botanical Physiology.

M. Palisot de Beauvois has communicated to the Class the result of an experiment, calculated to enlarge our knowledge upon the subject of the sap of vegetables.

Instead of simply removing a strip of bark from around a branch, as is generally done, he entirely isolated a square piece, making a cut all around, and in such a way that its fibres had no longer any communication with the rest of the bark, above, below, or laterally. He also removed the liber and cleansed the cambium thoroughly, leaving the wood only untouched at the bottom of the cut. The edges of this piece of bark, thus isolated, did not cease to exhibit vegetation, as well as the bark of the external edge of the cut; in some trees it even gave out a well-organized bud. Nothing can prove more strongly the general communication of all the parts of a vegetable, and the manner in which they mutually supply each other in their functions; for this piece of bark could only draw its sap from the wood concealed under it.

In our Report for 1806, we have detailed the peculiar opinion of M. de Beauvois on the fecundation of the mosses, and we cited at the same time the objections which still prevent several botanists from adopting this opinion, which consists in regarding as the pollen or fecundating powder the green dust which fills the urn of the mosses; and as the seed, another dust which M. de Beauvois places in a capsule situated in the axis of this very urn; whereas Hedwig takes the green dust for the seed, and looks for the pollen in other organs; and some more modern botanists will not even admit of distinct sexes in these plants, regarding this dust as heaps of small bulbs, or buds.

M. de Beauvois has this year made a discovery which confirms him in his opinion. Having carefully examined the urn of the *mnium capillare*, he found, 1st, That the green dust of the urn did not adhere to the central capsule, as it would have done if it had been the seed, and if this capsule was a columella, as the followers of Hedwig assert. 2. That there were in the capsule transparent grains, larger than those of the green dust.—3. That in the green dust itself there were grains of two kinds, some green, opaque and angular, united by threads; others transparent and spherical.

M. de Beauvois, on afterwards examining the dust of the lycopodii, also found two kinds of grains in them; some were opaque and yellow, and others round and transparent, like

like drops of water; and at most in the proportion of one to thirty with respect to the former.

M. de Beauvois, who regards the opaque grains as the pollen, thinks that the transparent bodies which are mixed there are a species of buds or bulbs, calculated to send forth new plants, and that it was these which germinated when Hedwig and other observers obtained young plants by sowing the dust of the lycopodii and the mosses: these experiments can therefore no longer be opposed to him.

With respect to the true grains, they are placed according to him in a different manner in the lycopodii and in the mosses: the axillæ of the leaves of the lower part of the ear contain in some plants of the former family, small capsules, each containing some grains larger than the dust of the upper capsules, which have been considered as seeds by Dillenius, and by all those who, like him, regarded the dust as a pollen.

M. Willdenow regards them as a species of bulbs; and it is the common opinion of those who will not admit any sexes in the mosses, the lycopodii, and the other cryptogamia.

But M. de Beauvois finds that these grains have all the characters of organization assigned to the seeds by the most accurate botanists, and that consequently we cannot hesitate to regard them as such, although they have not as yet been discovered in all the lycopodii: he admits, nevertheless, that he has not succeeded in raising them, but this he ascribes to their having been in too fresh a state.

We briefly noticed in our two former reports, the discussions between our associates, Mirbel and Richard, on the internal composition of the grain of certain vegetables. As these discussions had nothing less in view than the subversion of all the present systems, they were conducted with a warmth proportionate to their importance, and it becomes necessary for us to give some account of the point at which the dispute has arrived. For this purpose we must take up the subject a little earlier.

When we immerse in water a bean, for example, it soon splits, and at the junction of the two lobes which form the greater part of its mass, we observe on one hand a small fleshy-like body of a conical figure, and on the other two small leaves equally distinct. If we had made this bean germinate, the conical part would have entered into the ground and would have formed the root, the two small leaves would have risen into the air, and from them the rest of the plant would have been continued; the two large lobes

lobes adhering at the point of junction of the two other parts, after having for some time performed the part of the leaves, would be soon dried and disappear.

The small conical tubercle bears among botanists the name of *radicule*, the part opposite which, by developing itself, gives the entire trunk of the plant, is called *plumule*, and the two lateral lobes are called *cotyledons*.

Numerous experiments have shown that the functions of the cotyledons consist in furnishing the substance necessary to the first development of the plumule and the radicule, until the small plant is strong enough to extract from the earth and from the atmosphere the juices adapted for its ulterior growth.

Observations not less numerous have shown, that plants with two cotyledons, which are the most numerous in nature, have a great number of common characters, and that they differ mostly in the details of their organization from those which have only a single cotyledon, and still more from those where no cotyledons are observable: consequently botanists have formed from this composition of the small vegetable embryo, the basis of their first division of plants.

M. Desfontaines, in a memoir of which we formerly gave an analysis, seemed to have put a seal upon this division, by proving that the ligneous trunks of the dicotyledonal plants have another internal texture, and another manner of growing than those of the mono-cotyledones and the acotyledones.

But as it frequently happens in natural history, particularly when the fundamental characters rest upon empirical observations only, and the rational relations of which with the rest of the organization have not been gradually appreciated, these rules are not without exception. It has been discovered that in the seeds of certain plants, which throughout their whole structure resemble the dicotyledones, or those which have no cotyledons, or which have more than two, exceptions in an inverse ratio have been supposed to exist, and these notions have excited the attention of botanists more than ever to the examination of the seeds of all the plants. In the course of this investigation, however, some were found, the structure of which appeared problematical, and in which the same organ received different names according to the way in which each was regarded.

The *nelumbo* is one of the most remarkable of these doubtful species: it is an Indian plant, which has a considerable resemblance to our nemiphar: its seed contains a body

body divided into two lobes; at two-thirds of its height and between these lobes, there is a small membranous sac, from which proceed the first leaves, and it is only after the stalk which bears these leaves is a little lengthened, that it produces laterally some small roots.

Messrs. Mirbel and Poiteau, in conformity to a resemblance at least apparent, have asserted that the two lobes are the two cotyledons; that the first leaves form the plumule, and the sac which envelops them is a kind of sheath: that the radicle remains inactive and without development, and that the fibres which arise from the small stalk are analogous to those roots which issue from rampant plants.

M. de Mirbel, in particular, thinks that he has found in the interior of these lobes, a set of vessels entirely similar to those of the cotyledons in the plants which have double cotyledons. These two botanists have therefore ranged the nelumbo among the dicotyledones.

M. Richard, on the contrary, maintains that it is the small sac which ought to be considered as the only cotyledon, and that the two lobes belong to the extremity of the radicle: he compares these bodies with those observed in other embryos, and to which he has given the name of hypoblasti, being the same which Gærtner called vitellus; and this analogy appeared to him to be the clearer, because the lobes in question, as well as the other hypoblasti, exhibit no growth at the time of germination, and the contrary is the case with most of the cotyledons. The lateral production of the roots is a natural and general consequence of the presence of a hypoblastus, which hinders the radicle from shooting out in a straight line. According to this reasoning, M. Richard has classed the nelumbo among the mono-cotyledones.

The discussion afterwards turned upon the nature of these hypoblasti. M. de Mirbel has compared what M. Richard calls by this name among the grasses, and which is the scutellum of Gærtner, with the cotyledon of the asparagus and some other plants which have only one, and has concluded from the comparison, that the hypoblastus of the grasses is precisely their cotyledon: this would give him the advantage of all the analogies cited by M. Richard.

M. Poiteau has also given in a memoir upon this question, taking the same side with M. Mirbel.

M. Richard, in reply, maintains that there is a greater difference than M. Mirbel is aware of; that the plumula of the asparagus and of the other plants mentioned, is enve-

loped

loped in the cotyledon; that it pierces through it in growing, and that this is a character essential to the plumula of all the monocotyledonal plants: that in the grasses, on the contrary, the plumula is enveloped in a tunic in the form of a cone distinct from the hypoblastus, and that it is this tunic, which enveloping the plumula, ought to be the true cotyledon; but M. Mirbel has only shown in this small cone an excrescence, resulting from the plumula assuming in the grain an increment, proportionally stronger in the grasses than in the other monocotyledons.

Arguments drawn from plants more or less resembling the nelumbo have been adduced on both sides.

M. Mirbel has shown, that there exists a great resemblance between the grains of the pepper plant and some others clearly distinguishable as dicotyledons, in the structure of their suckers and the grains of the nelumbo. In truth we do not see in the nelumbo, nor in the nymphæa, the annual ligneous layers which distinguish the dicotyledons; but it is to their loose texture, according to M. Mirbel, that we ought to ascribe this difference.

M. Richard has produced in his favour the families of the hydrocharideæ and the hydropeltideæ, which he thinks the nelumbo and the nymphæa most resemble, and several genera of which have thick hypoblasti, in a hollow of which is lodged the plumula, enveloped with a cotyledonal purse, although these hypoblasti are not divided so deeply as in the nelumbo.

But at the same moment with this partial discussion another arose: two or three years ago M. Richard, ascertaining that the division of the plants, according to the number of their cotyledons or seminal lobes, is in some cases obscure or even insufficient, proposed a new one, taken from another part of the embryo, viz. from the structure and envelope of the radicle.

In the plants commonly called dicotyledonal, the radicle or the small conical tubercle mentioned above, becomes of itself, by shooting out, the root of the vegetable: in others, it is only a small sac containing tubercles, which become the roots.

M. Richard denominates the plants of the first form *exorhizes*, and those of the second *endorhizes*.

M. Mirbel asserts that this new division is still less applicable than the ancient; that, in truth, the radicle of the grasses is conformed to this description of the *endorhizes*, but that in the other monocotyledons there is no appearance of sac, but a small node at the basis of the nascent

 root,

root, and that this node is re-produced in plants analogous to the dicotyledons, such as this very pepper to which he had already had recourse in the dispute relating to the nelumbo.

Here M. Richard affirms that the pepper is wholly monocotyledonal, as well as the nelumbo; and it may happen, according to him, that we may overturn the structure of the stalks of the family of the pepper plants, or that we may be obliged to refer to the general rule of the structure of the stalks, new determinations calculated to render its application more precise, and to banish the semblance of exceptions.

[To be continued.]

LXXX. *Intelligence and Miscellaneous Articles.*

INTERIOR OF AFRICA.

A German of the name of Roentgen has been recently prosecuting the same objects of discovery that excited the ardour of the celebrated though unfortunate Park; and, penetrating into the central regions of Africa, to reach, if possible, the city of Tombuctoo, which has never yet been explored by any European traveller. The following article on this subject has appeared in a German journal.

"There has been lately published at Neuwied, an interesting letter from the traveller Roentgen to his brother. It reached him through Professor Hagen, who received it from Mr. Nunemann, of London. Roentgen, it appears, after visiting Paris, Vienna, and London, had repaired to Mogadore, where he resided a considerable time; and the letter in question, dated the 21st of July 1811, was written on the bank of the river Teusifft, at the moment of his departure for the interior of Africa. The following is some of the most interesting information it contains:

"During my residence at Mogadore, I was engaged day and night in studying the Arabic, and I have succeeded in making myself understood by the natives of the country. I will avail myself of that knowledge of the country, and of the manners of the people, which I have acquired, in order to travel directly to Tombuctoo. I would not act with so much boldness, were I not convinced that Providence has destined me to make the discovery of the interior of Africa. My good stars have furnished me with a companion in my travels, than whom I could not have wished for a better. He is a German, who, when only twelve years old, quitted his paternal roof, having an irresistible inclination

inclination for roaming; he has never since lived six months on the same spot, and is now 38 years of age. He knows all the European languages,—the Slavonic excepted. Fourteen years ago, when destitute of money or protection, he was impressed by the English for a sailor, in an island of the Mediterranean, where he happened to be; he was inhumanly treated by them, and reduced almost to despair. His ship anchored before Tetuan, for the purpose of watering; and there having struck an English officer, who had used him ill, in order to avoid punishment he escaped, and became a Mussulman at Tetuan. Since then, he has traversed the Barbary States in all directions, and has lately returned from a pilgrimage to Mecca. He has lived at Jamba, in Africa, as a coffee-house keeper, and at Janoi as a physician. At Constantinople he has superintended the gardens of a pacha. I got acquainted with him at a merchant's in Mogadore, who had hired him as a gardener. I have taken him into my service, and I treat him rather as a friend than as a domestic: the benefits which I shall derive from his experience are immense.

"About a month ago I travelled with a caravan of merchants to Morocco, where I procured valuable information respecting the communications with the interior of Africa.

"It is impossible to convey an idea of the violent hatred which animates the Moors against Christians. Even at Mogadore, I could hardly go abroad without being overwhelmed with insults. I was obliged, in order to view the city of Morocco, to get an escort of four soldiers, who, by order of the Government, were to keep back the populace. Even then I was often assailed by stones, one of which hit me so severe a blow on the forehead, that for some time I thought myself dangerously wounded. This hatred of the Moors arises in a great degree from our dress.

"I saw at Morocco preparations for the setting out of a caravan, which was to reach Tombuctoo by Tafilet and Tunt. I immediately formed a resolution to join this caravan, and I returned to Mogadore. My companion was delighted with the plan, which I did not communicate to any one else but to one Christian. I caused it to be reported at Mogadore, that, disgusted with the bad treatment I had received at Morocco, I meant to repair to Tangier, and from thence embark for Gibraltar. This pretended project furnished me with a pretext for purchasing a mule and every other necessary for my journey. I secretly procured some Moorish garments. Having finished my preparations, I invited some Christians at Mogadore to a party of pleasure on a mountain about six English miles off, whither they were often in the habit of going. I have there spent one day with them, and declared that I meant to proceed directly for Tangier. They will accompany me to a certain distance, and will give out at Mogadore that I am on my way to Tangier. As soon as I am left alone with my fellow-traveller, I mean to clothe myself in my Moorish garb, and to enter the great road which leads from Tafilet to Morocco. From thence I shall reach Deminit, a town situated at the foot of Mount Atlas, where I shall be safe from any searches which the Governor of Mogadore might make, should he learn that I have not gone to Tangier. At Deminit I shall join a caravan which will pass there about that time, and with it I shall cross Mount Atlas covered with snow, and next enter the burning plains of Tafilet. I shall remain at Tafilet with a German renegado: there are in that city a number of Germans. There are some Germans in Morocco, and to one of them I am indebted for some valuable information. I expect to find a German in Tombuctoo, and there I mean to remain six months, making it the centre of my observations on the interior of Africa. I shall pass for a physician; I have laid in a supply of medicines, of which I know the application. It is my wish to penetrate towards the south, and to be able to reach Wesemb, or the Cape. Should I find this too difficult, I mean to return to Europe, to publish the Journal of my travels; and shall again return to Africa, where I am destined to make some discoveries."

LECTURES.

Dr. Clutterbuck will begin his Spring Course of Lectures on the Theory and Practice of Physic, Materia Medica, and Chemistry, on Monday, January 18th, at Ten o'clock in the Morning, at his House, No. 1, in the Crescent, New Bridge Street: where further particulars may be had.

Theatre of Anatomy.—Lectures on Anatomy, Physiology, Pathology, and Surgery, by Mr. John Taunton, F.A.S. Member of the Royal College of Surgeons of London, Surgeon to the City and Finsbury Dispensaries, City of London Truss Society, &c.

In this Course of Lectures it is proposed to take a comprehensive view of the structure and œconomy of the living body, and to consider the causes, symptoms, nature, and *treatment of surgical diseases,* with the mode of performing the different surgical operations; forming a complete course of anatomical and physiological instruction for the medical or surgical student, the artist, the professional or private gentleman.

An

An ample field for professional edification will be afforded by the opportunity which pupils may have of attending the clinical and other practice of both the City and Finsbury Dispensaries.

The Winter Course will commence on Saturday, January the 23d, 1813, at Eight o'clock in the Evening *precisely*, and be continued every Tuesday, Thursday, and Saturday, at the same hour.

Particulars may be had, on applying to Mr. Taunton, Greville-Street, Hatton-Garden.

LIST OF PATENTS FOR NEW INVENTIONS.

To Edward Jukes, of Walworth, in the county of Surrey, gentleman, for his improved shears for pruning of trees, gathering grapes, and other fruit, and for cutting off such limbs as may be injured, and thereby more easily destroy the insects occasioned by blights, which he denominates an Averruncator.—7th November, 1812.

To Joseph Bramah of Pimlico in the county of Middlesex, engineer, for certain improvements in the construction of various parts of wheeled carriages, one of which improvements is applicable to other machinery where a rotatory motion is necessary.—26th Nov.

To Henry Osborn of Bordesley, near Birmingham, in the county of Warwick, for a new method of welding and making of various kinds of cylinders of iron and steel.—28th November.

To Thomas Rogers of the city of Dublin, Esq. for his method of constructing wheels for carriages.—28th November.

To Charles Price of the Strand, in the county of Middlesex, umbrella maker, for his parasol and umbrella on an improved construction, which he denominates "The Improved Solumbra."—4th December.

To Samuel Smith of Coventry, in the county of Warwick, watchmaker, for an improved escapement for watches, by an invention calculated to make them beat dead seconds for principle and parts of seconds, by means of clock pallets attached to a lever operating on a vertical wheel.—4th Dec.

To Robert Were Fox the younger of Falmouth, in the county of Cornwall, merchant, and Joel Lean the younger, of the parish of Budock, near Falmouth aforesaid, gentleman, for certain improvements on steam engines, and the apparatus needful or expedient to be used with the same.—10th December.

To

To John Spencer of Port Ballantrus, in Ireland, salt manufacturer, for his addition to, or improvement in, the setting up of salt pans.—14th December.

To Joseph Hamilton of the city of Dublin, gentleman, for certain new methods of applying well known principles in the construction and formation of earthen wares.—16th December.

To John Hanbury the elder, of Bartlett's Buildings, Holborn, in the city of London, warehouseman, for his method of weaving carpets, commonly called Scotch or Kidderminster, by which a new and firmer texture and larger patterns can be produced.—19th December.

To Thomas Rogers of the city of Dublin, Esq. for his method of applying manual powers to the crane, pile-driver, and other machinery.—19th December.

To John Fisher of Oundle, in the county of Northampton, ironmonger, for his invented article for preventing chimneys smoking, which he has named a "Smoke Conductor," and which may be manufactured either in cast-iron, wrought iron, copper, brass, and tin, or any other metallic substance.—19th December.

To George Heffer of Carlisle Place, Lambeth, in the county of Surry, coach-maker, for an improvement in the construction of four-wheeled carriages.—19th December.

To John Morgan of York Street, in the city of Dublin, M. D. for a new power applicable to the propelling of vessels and boats of every description through the water, and also to the pumping of them.—19th December.

To Jacob Samuel Eschanzier, of Gibraltar, and Henry Constantine Jennings, of Marchmont-Street, Russell-Square, in the county of Middlesex, gentlemen, for their new mode of manufacturing, using, and applying certain articles by means of which mariners and other persons may be saved from drowning.—19th December.

To John Lewis, of Llanelly, in the county of Carmarthen, assayer of metals, for certain improvements in the art of smelting copper ore.—19th December.

To John Barber, of St. Mary Street, Portsmouth, in the county of Southampton, sword cutler and surgical instrument maker, for a new instrument of great practical utility to surgeons; namely, an instrument whereby they may with the utmost facility stay and prevent the hæmorrhage of the subclavian artery safely in cases when necessary to amputate the arm from the shoulder joint.—21st December.

METEORO-

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND,

For December 1812.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Nov. 27	45	47	44	30·10	0	Foggy
28	44	47	41	·01	0	Foggy
29	42	46	47	29·90	7	Cloudy
30	47	50	47	·81	0	Cloudy
Dec. 1	47	52	46	·70	0	Small Rain
2	47	53	47	30·00	0	Ditto Rain
3	47	49	45	·10	0	Ditto Rain
4	46	47	40	·11	0	Cloudy
5	40	45	32	·09	6	Fair
6	38	38	30	·40	7	Fair
7	29	35	29	·58	6	Fair
8	26	30	25	·26	10	Fair
9	23	32	30	29·89	7	Fair
10	30	33	39	·90	4	Fair
11	31	32	29	·91	0	Small Snow
12	28	32	26	·82	0	Cloudy
13	25	33	26	·70	17	Fair
14	27	33	29	·70	7	Fair
15	31	32	29	·40	16	High Wind
16	27	28	32	·01	0	Cloudy
17	33	33	32	28·92	0	Snow
18	33	37	37	29·21	0	Rain and Snow
19	38	39	36	·48	0	Small Rain
20	36	37	33	·51	5	Cloudy
21	32	35	33	·80	0	Foggy
22	37	38	33	.83	0	Foggy
23	34	36	33	30·18	4	Cloudy
24	32	35	32	·35	6	Cloudy
25	30	33	32	·42	5	Cloudy

N.B. The Barometer's height is taken at one o'clock.

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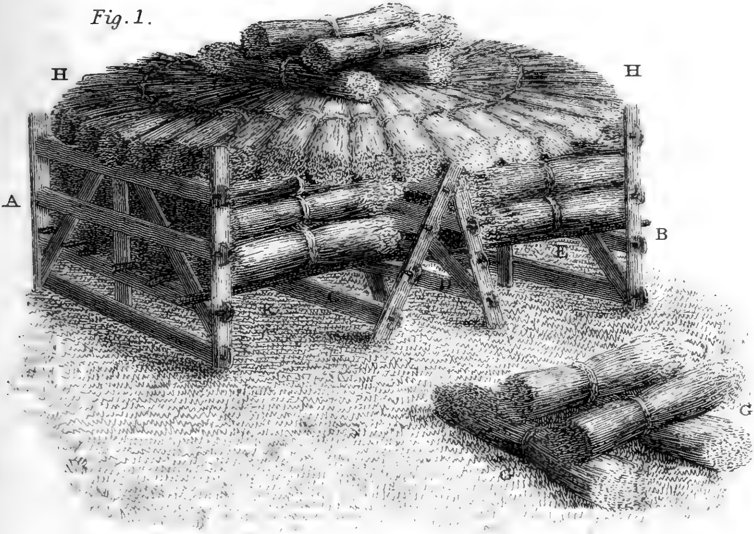
END OF THE FORTIETH VOLUME.

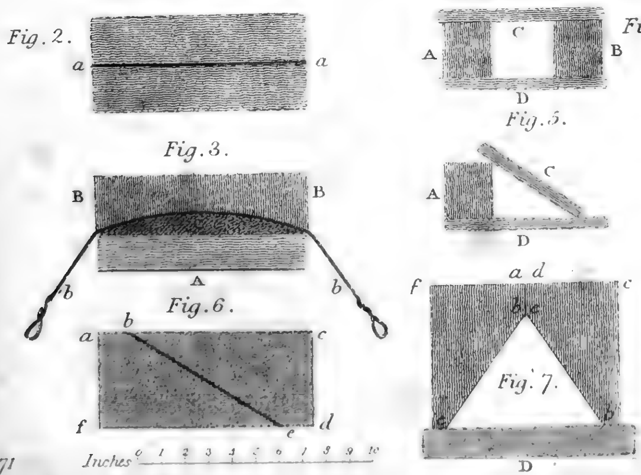
Printed by Richard Taylor and Co., Shoe Lane, London.

Foundation of Mr. Wm. Jones's Temporary Corn Rick.

Fig. 1.



Mr. Stephen's Method of dividing Bricks.



S. Porter sc.

Section of Mr. Wm. Jones's temporary Corn Rick.

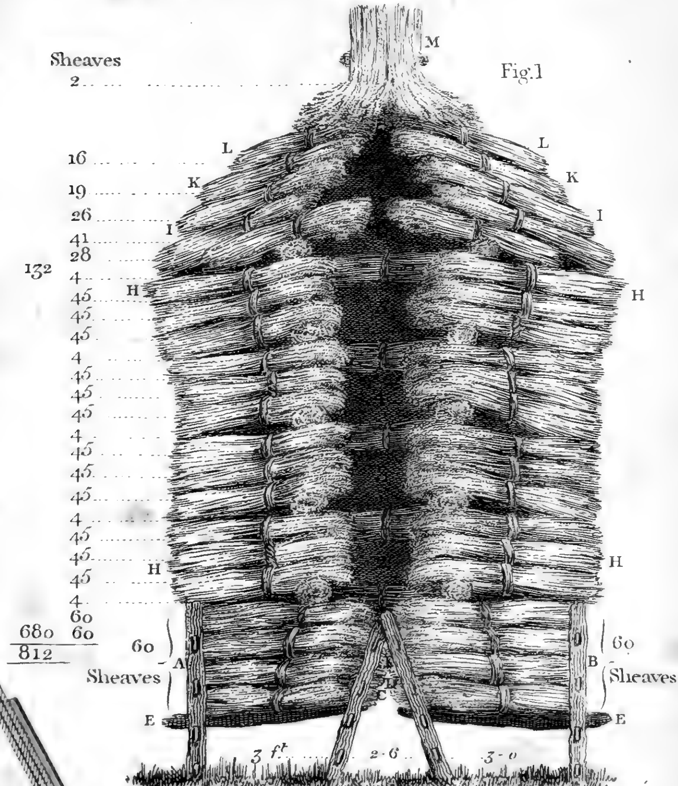


Fig. 1

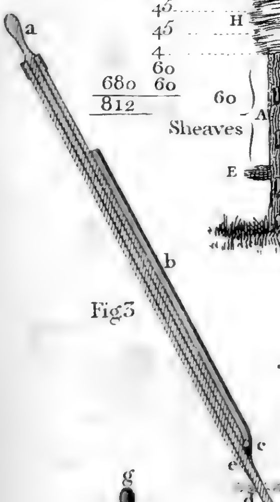


Fig. 3

Mr. Waistell's Improvement on the Dibble, or Tool for planting Acorns in Bushes.

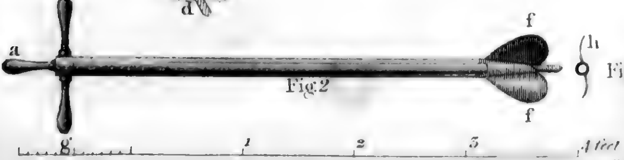


Fig. 2

Fig. 4

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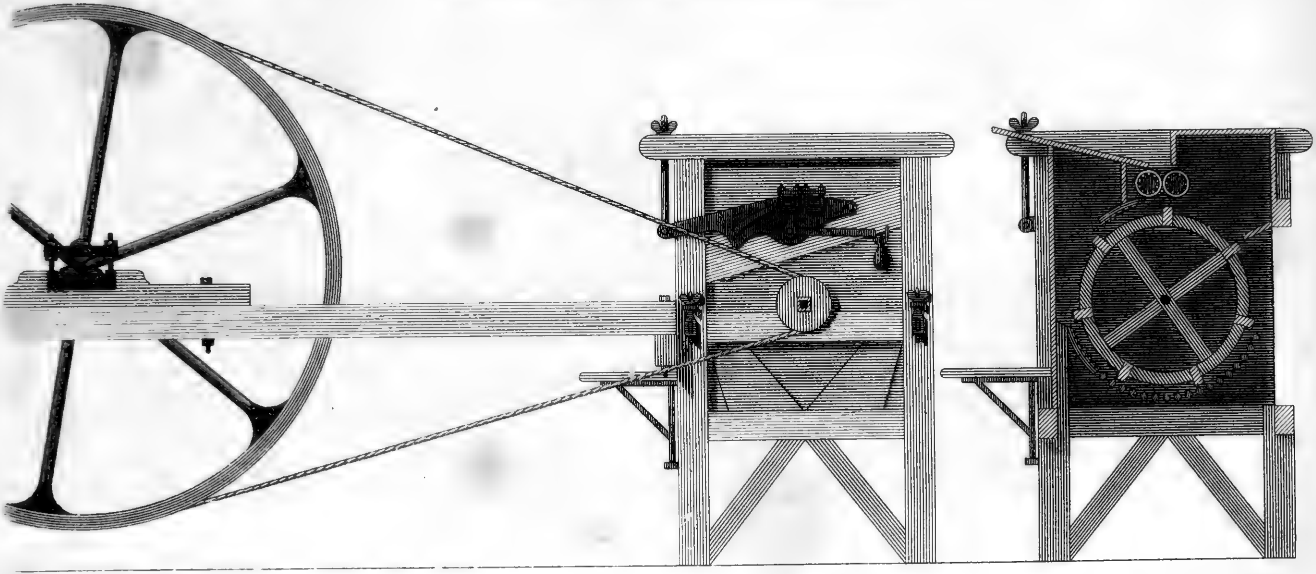
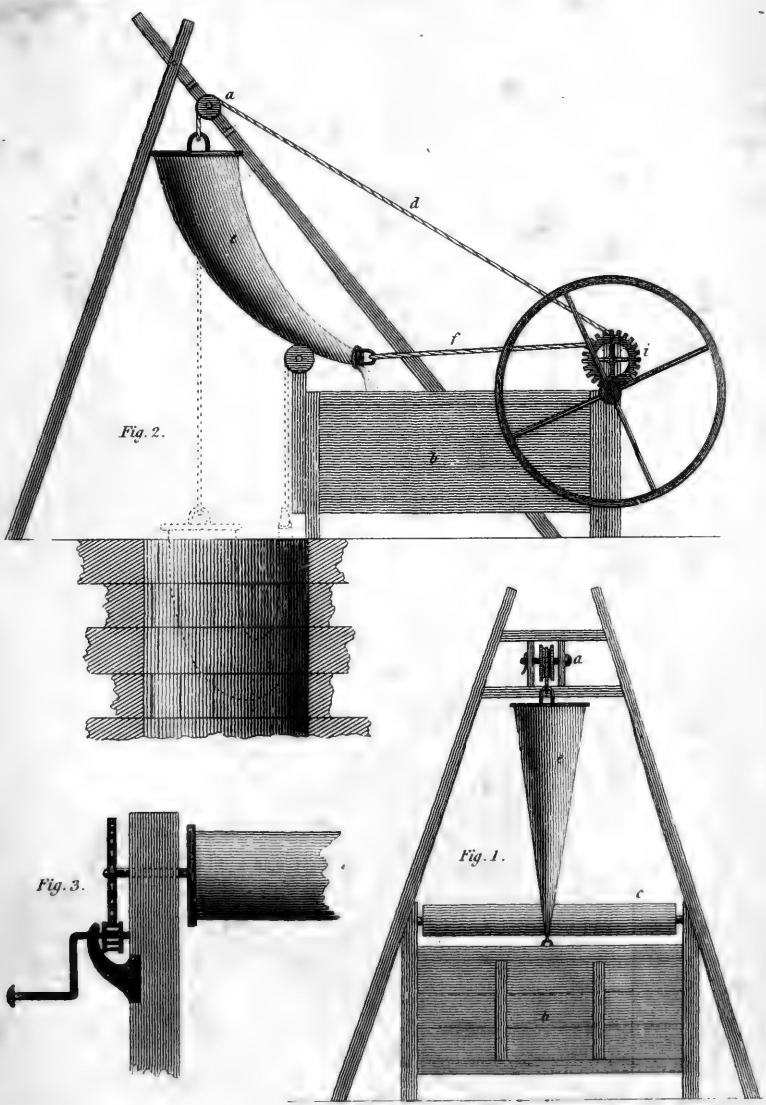
S. Porter sc.

Mr. Tawney's Thrashing Machine.



S. Porter sc.

Machine employ'd in the Levant to raise Water.



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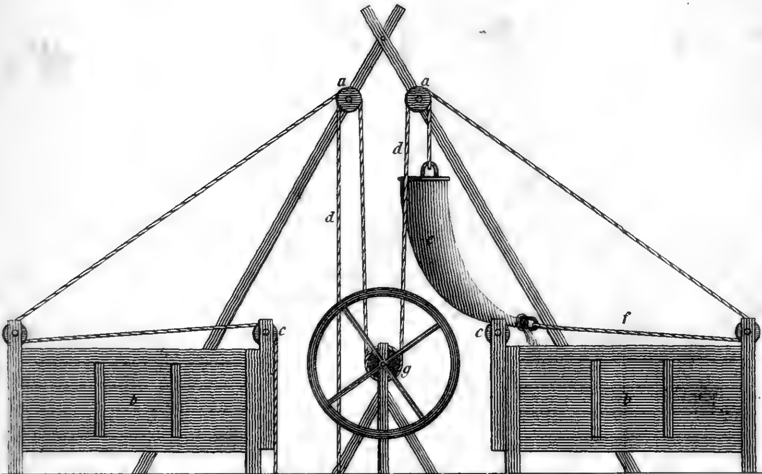


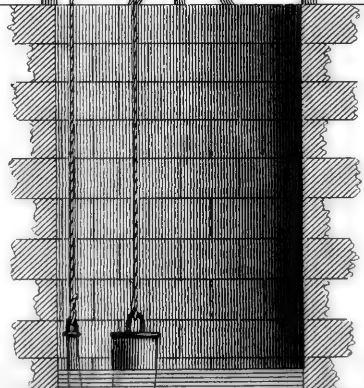
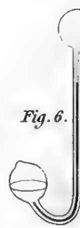


Fig. 5.

Leslie's differential Thermometer.

Fig. 6.



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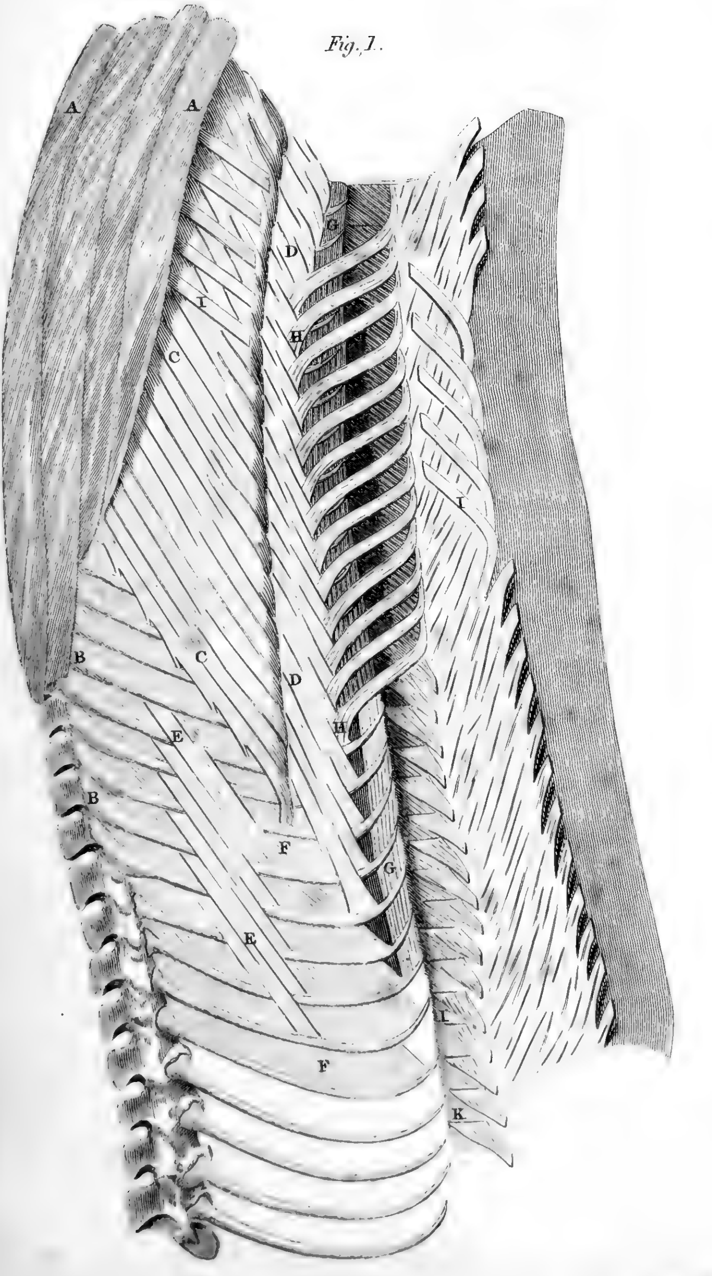

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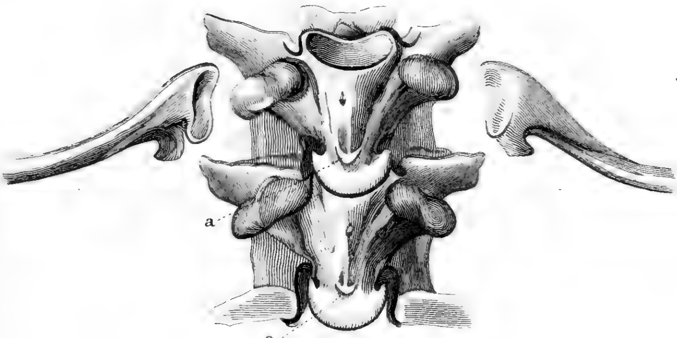

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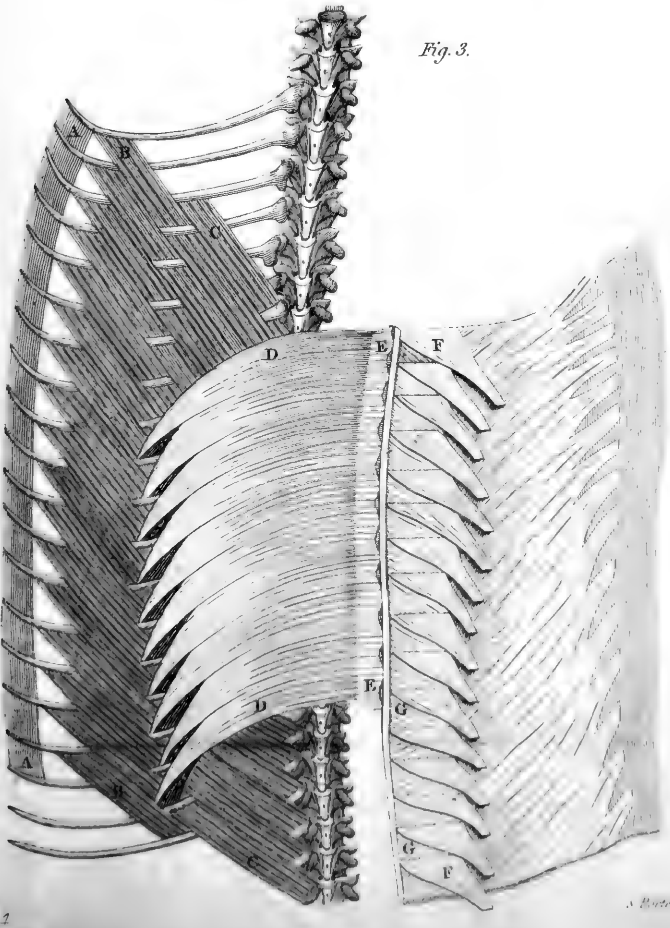

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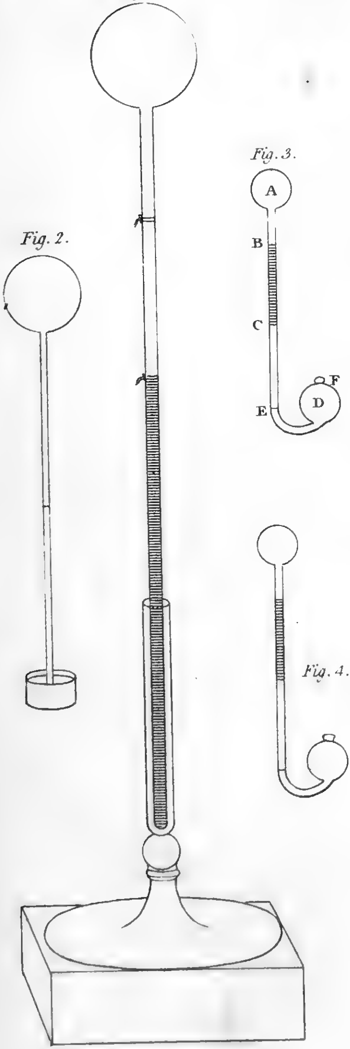

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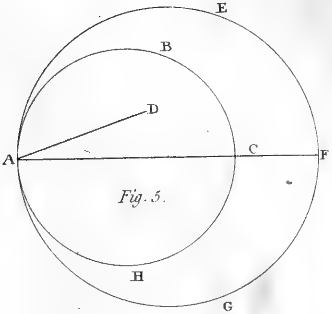

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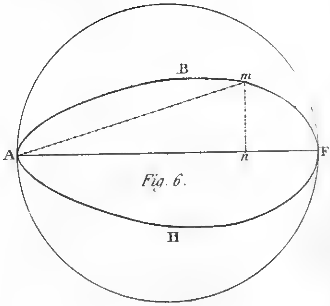

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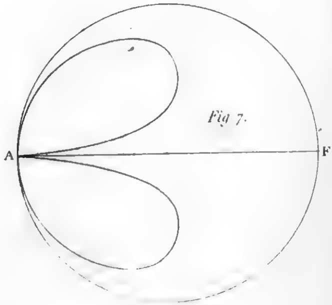

Fig. 7.

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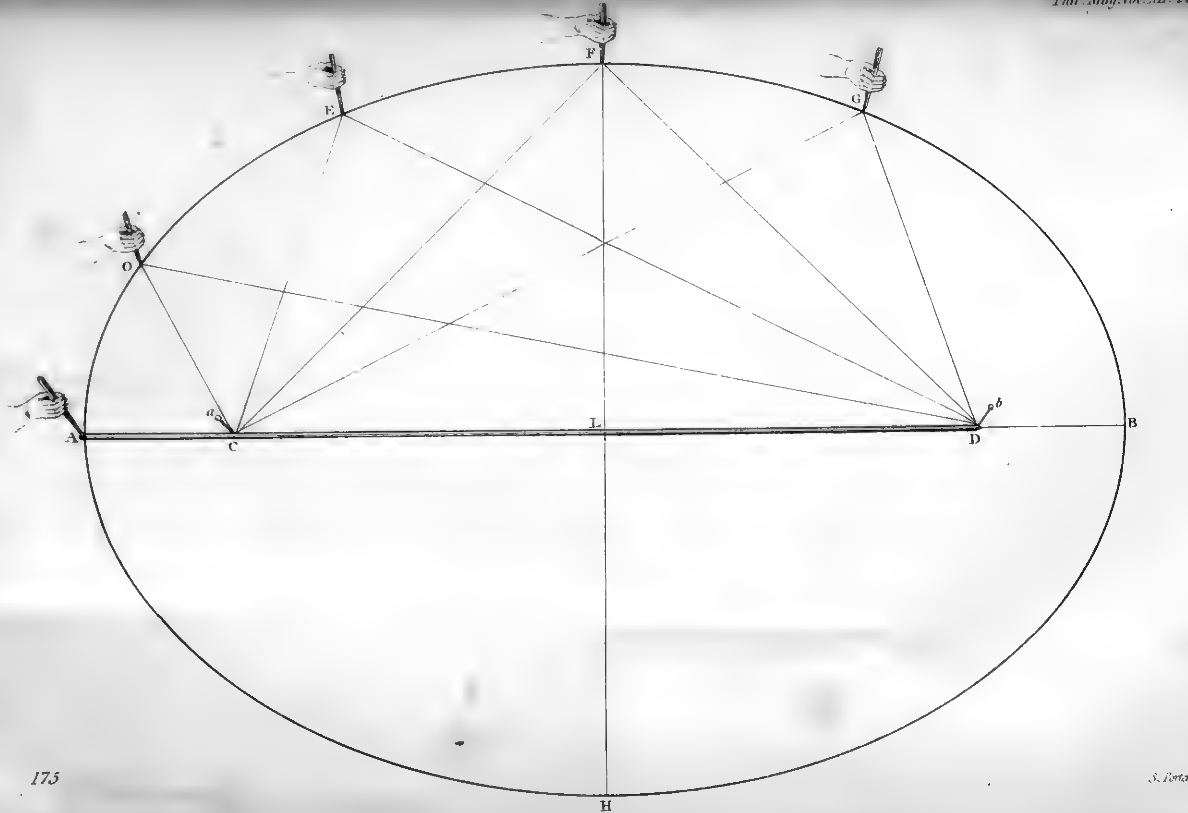

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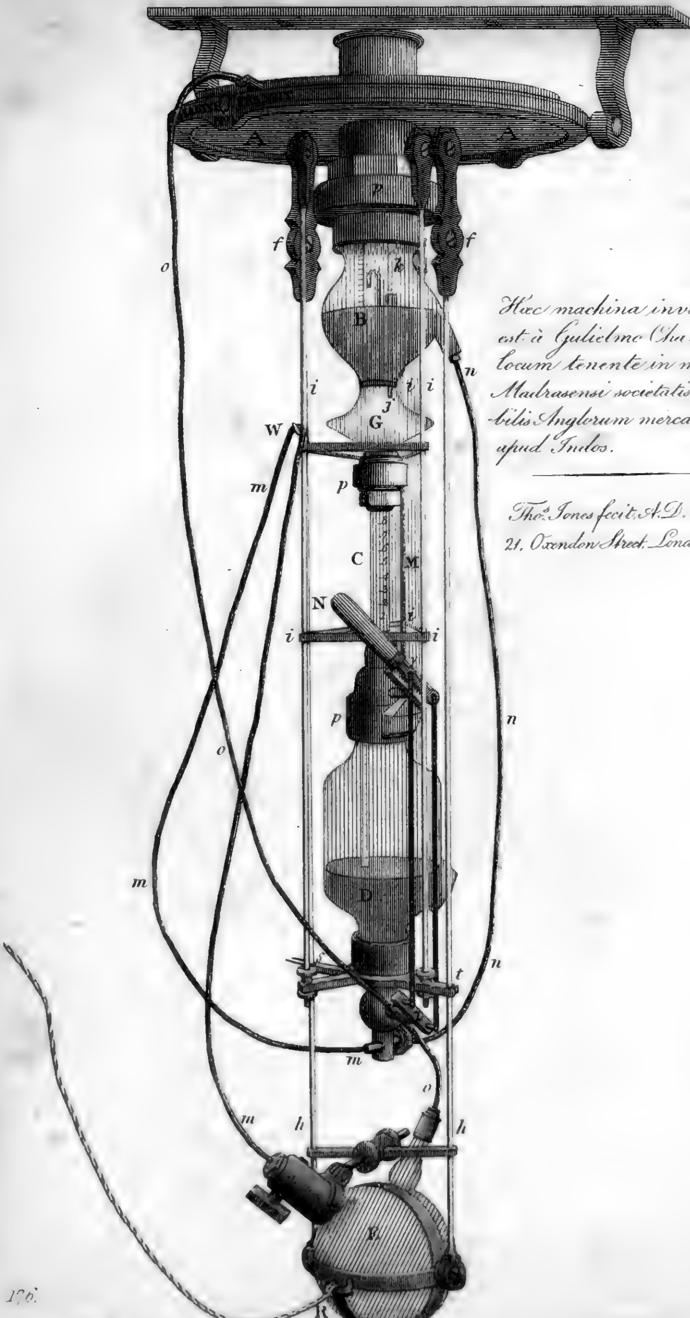


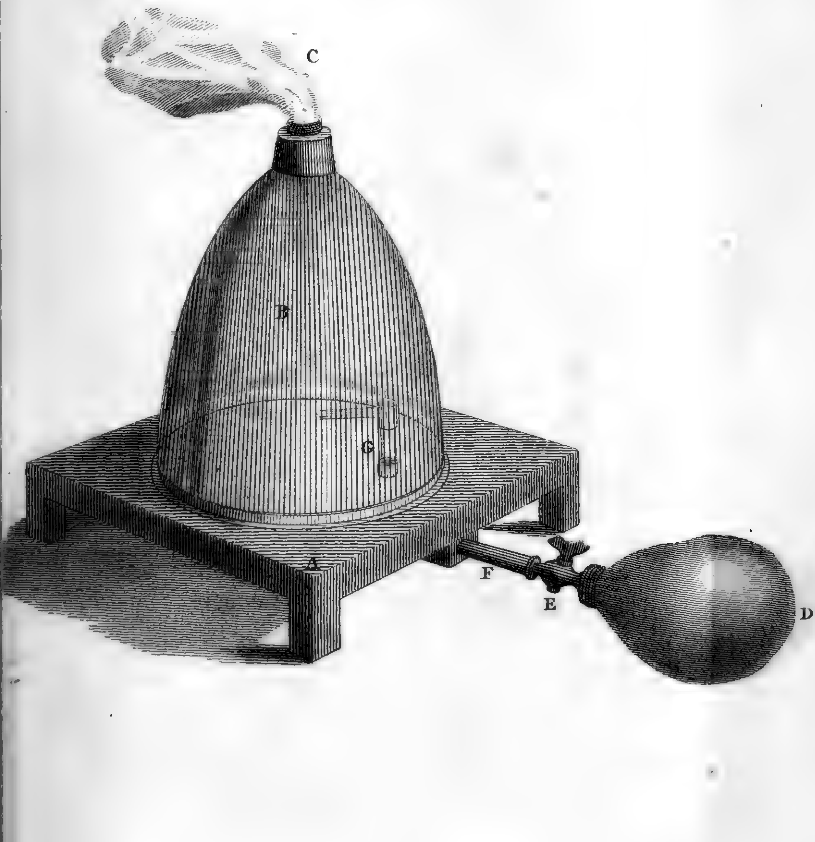
Hæc machina inventa est à Gulielmo Chavasse locum tenente in militiâ Madrasensi societatis honorabilis Anglorum mercatorum apud Indos.

Thos. Jones fecit. A. D. 1812, 21, Oxendon Street, London.

176.

Drawn by
M. I. Harris 8th Regt. Madras N.I.

Engraved by
S. Porter, Weston Street, Somers Town.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Vol. 40. Philosophical Magazine. July 1812.

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*** Communications for this Work, addressed to the Editor, Pickett-Place, Temple Bar, will meet with every attention.

R. TAYLOR AND CO. PRINTERS, SHOE LANE, FLEET STREET, LONDON.

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Dr. Pearson's *Answer to Dr.* Marcet *came too late for insertion in the present Number.*

*** Communications for this Work, addressed to the Editor, Pickett-Place, Temple Bar, will meet with every attention.

R. TAYLOR AND CO. PRINTERS, SHOE LANE, FLEET STREET, LONDON.

This Day was published, in Folio, Price 3s.

E METEOROLOGIST'S ANNUAL ASSISTANT IN KEEPNG A DIARY OF THE WEATHER; or, Register of the State Barometer, Thermometer, Wind, &c. and Fall of Rain.

London: Printed for Robert Baldwin, 47, Paternoster-Row.

NEW PHILOSOPHICAL JOURNAL,

e 1st of January will be published, No. I. Price 2s. 6d. and continued Monthly,

NALS OF MECHANICAL PHILOSOPHY, CHEMISTRY, AGRICULTURE, AND THE ARTS.

By THOMAS THOMSON, M.D.

. L. and E. Fellow of the Linnæan Society, of the Geological Society, Wernerian Natural History Society, and of the Imperial Chirurgo-al Academy of Petersburgh.

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ENGRAVINGS.

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*** Communications for this Work, addressed to the Editor, Pickett-Place, Temple Bar, will meet with every attention.

R. TAYLOR AND CO. PRINTERS, SHOE LANE, FLEET STREET, LONDON.

Mathematical and Scientific Education.

MR. FIRMINGER, Assistant Astronomer at the Royal Observatory, Greenwich, from the Year 179[illegible] to 1807, has commenced an Establishment for a limited Number of Young Gentlemen, resident in his House, Charles Street, Clarendon Square, Somers Town, for Instruction in the various Branches of the Mathematics and Mathematical Sciences, with their practical application to the several Departments of Civil, Military, Naval, and Commercial Education.

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A Series of Astronomical Observations will be made, including, besides such as are taken in conducting the Business of a Regular Observatory, those of Lunar Distances, Eclipses of Jupiter's Satellites, and all Observations requisite in determining the Longitude at Sea or Land, the Rates of Time-keepers, &c. Whereby the Students of Navigation and Astronomy have the advantage of acquiring a complete knowledge of the Practical as well as Theoretical Departments of these Sciences.

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The Works of Hutton, Playfair, Atwood, the Cambridge and different Courses of Mathematics, with such Authors, English and Foreign, as afford the best information, will be Books of Reference and Study in the respective Departments of Science which they contain.

The above Course of Studies and Establishment is calculated (and it is hoped will prove to those who honour it with their patronage) to afford the most effectual means for the Education of Young Gentlemen in the several branches of Instruction which are professed to be taught; and by which they will be prepared for the Professions of the Army and Navy, for their entrance into the Public Colleges and the Universities, and for any Department of Public or Private Life, wherein, besides a Classical, a Mathematical and Scientific Education is required.

Young Gentlemen preparing for their entrance at the University at Cambridge are instructed in the Elements of Euclid by Simson, the Cambridge Mathematics, and the Courses of Study adopted in the several Colleges of that University.

The above is, in part, the Outline of a Plan of Instruction drawn up about four years since by Mr. Firminger, for the late Right Hon Charles Greville, which was to have been adopted under Mr. Firminger's superintendance in a large Establishment preparing for that purpose at Milford in South Wales.

A most Complete and Elegant Observatory was nearly finished, and a set of Astronomical Instruments, purchased at great Expense, was provided in readiness for commencement, when unfortunately the Death of Mr. Greville, which happened suddenly, put a premature close to the undertaking, as sufficient funds (undoubtedly owing to so unexpected a circumstance) had not been provided for its completion.

Mr. Greville, whose sources of information were widely extended, and whose abilities to appreciate were acknowledged of superior merit, being fully impressed with a conviction that much was wanting in Schools of Education, and that by a proper allotment of their time the Elèves for Instruction might, in the usual period allotted to a Classical Education, acquire also a considerable degree of Mathematical and Scientific Knowledge, Departments of Instruction he held, in the present enlightened state of Society, equally necessary in forming the Public and Private Character, had determined that nothing should be wanting in his Establishment to complete his intention of making it subservient to the acquirement of Mathematical and Scientific as well as of Classical Knowledge, for which purpose every necessary provision would have been made for the Experimental as well as Theoretical Departments of Science, had he lived to complete his intentions. As a more particular account of his intended Establishment and Plans of Education may be interesting and useful to the Public, an extended account will be given in a future Number of this Work, with a Plan and Elevation of the Observatory, which at the same time combines an Elegance of Form with Simplicity and Convenience, certainly not surpassed if equalled by any of the Observatories now extant.

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